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ARTICLES

415 **From Confusion to Clarity: Issues and American Voters, 1956–1968.** Analysis of national election surveys from 1956 to 1968 reveals significant changes in the voters' perceptions of issues and the major parties. There has been a considerable increase in the correlation of party identification and opinion on six major issues, relating to social welfare, racial integration, and foreign aid. Voters are more prone to see a difference between the parties on these issues and are increasingly likely to identify the Democratic party as favorable to federal governmental action. These findings contrast with those of *The American Voter* and similar studies. The reasons for the changes cannot be found in demographic factors, as tested by controls for age cohorts, education, region, and race. More probably the explanation lies in strictly political factors. A particularly important event was the presidential campaign of 1964, in which ideological differences between the parties were deliberately emphasized. The electorate responded to this campaign by becoming more ideologically aware, and its learning appears to have persisted through the 1968 election. This finding suggests that past conclusions about the low ideological awareness of the electorate were specific to the Eisenhower era, and that the issue content of the vote will vary with the stimuli provided by the general political environment.

By Gerald M. Pomper, Professor of Political Science, Livingston College, Rutgers University.

429 **Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election.** The importance of issues in deciding elections changes from one election to the next. As Key has shown, the issues of the role of the federal government in social life helped create the New Deal Democratic majority. In contrast, issues had only a marginal impact on the apolitical elections of the 1950s. Converse's technique of normal vote analysis reveals that issues were again highly related to the vote in 1968. This was particularly true of attitudes toward Vietnam, urban unrest and race, social welfare, and Johnson's performance as president.

Yet, even in an election in which issues appear important, some can have very different consequences for popular control of policy than others. On some issues, the electorate exercises no effective constraints on leaders' policy choices. On others (e.g., the escalation in Vietnam), the electorate permits leaders a wide array of options when a policy is adopted and passes a retrospective judgment on such choices in subsequent elections. Finally, on still other issues, the public may limit the options of leaders at the time a policy is adopted. The paper suggests the stringent conditions necessary for this type of popular control to exist.

By Richard W. Boyd, Assistant Professor of Government, Wesleyan University.

Comment. By Richard A. Brody, Professor of Political Science, Stanford University and Benjamin I. Page, Assistant Professor of Government, Dartmouth College.

Comment. By John H. Kessel, Professor of Political Science, Ohio State University.

Rejoinder. By Gerald M. Pomper.

Rejoinder. By Richard W. Boyd.

471 **A Theory of Neighborhood Problem Solving: Political Action *vs.* Residential Mobility.** People have three ways of responding to neighborhood problems: leaving (exit), political action (voice), and doing nothing (passivity). The model assumes: 1. Voice is more likely to ameliorate neighborhood problems than exit or passivity; exit, in fact, can make things worse, 2. Rational behavior on the part of residents, coupled with constraints that limit options: status, race, the responsiveness of government and the nature of the problems. Survey data on one city are combined with census data differentiating neighborhood types. Voice is characteristic of suburban areas among high and low status whites; exit is characteristic of white urban areas. Among ghetto blacks—whose exit options are severely constrained—voice is most characteristic. Problems faced by blacks and whites living in the city are similar, while their adaptations are different.

By John M. Orbell, Associate Professor of Political Science, University of Oregon and Toru Uno, Assistant Professor of Political Science, San Francisco State College.

490 **Some Conceptual Problems in Nuclear Proliferation.** While significant progress has been made in establishing a legal barrier to the further spread of nuclear weapons, some important nations

are withholding approval of this Non-Proliferation Treaty (NPT). Political factors are very significant, but agreement is made even more difficult by persistent ambiguities in technological forecasts of "how far we or they are from the bomb." The time-lag for any crash weapon programs will not remain as extended as one might hope, because civilian technology itself is drawing states ever closer to *de facto* military capabilities. Yet the length of this time-lag may be crucial to the maintenance of peace if political crises emerge. Since scientists offer widely varying estimates on such time-lags, a graphical formulation is offered to reduce ambiguity. In the end, the effectiveness of International Atomic Energy Agency (IAEA) safeguards under the NPT may depend more on their symbolic and political impact than on their technology.

By GEORGE H. QUESTER, Associate Professor of Government, Cornell University.

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498 **On the Concept of Ideology in Political Science.** Although the term "ideology" is ubiquitous in modern political discourse, it is used in diverse and usually ambiguous ways which limit its value as an analytical concept. The main ambiguity arises from the fact that, as most writers use it, the concept of ideology does not provide criteria for distinguishing ideological thought from nonideological thought. Lacking this power to make concrete discriminations, the concept fails to achieve empirical relevance. This paper attempts to remedy that deficiency and save the concept of ideology for the explanation of politics.

The problem of conceptualization is approached by viewing ideology primarily as a cultural phenomenon. As such, it is argued, ideology has characteristics that distinguish it from other symbol systems. Of special importance in this regard is the identification of basic differentia between ideology on the one hand, and myth and utopia (with which ideology is often confused) on the other. The features of ideology identified in this comparative analysis are then discussed in fuller detail with a view to understanding (1) the significance of ideology in politics, and (2) the way in which the concept of ideology can help us to understand politics, insofar as politics involves ideology.

By WILLARD A. MULLINS, Associate Professor of Political Science, Carleton University.

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511 **Preconditions of Mayoral Leadership.** Observers of city politics have often stated that political leadership—usually mayoral leadership—is a crucial ingredient in a city's ability to deal with its problems. And studies of successful leaders in urban systems have led to the formulation of a model of the "political entrepreneur" who is adept at accumulating political resources and pyramiding them to gain increased influence. Based on four years of observation of politics in Oakland, California, this study suggests that successful mayoral leadership and resource-pyramiding may be limited by governmental structure, the personality of the mayor, and the nature of the political system.

In Oakland, fragmented institutional authority and the council-manager form of government have created obstacles for elected officials. A privately oriented, "nonpolitical" mayor has avoided publicity and has tended to underutilize, rather than pyramid, his resources. Finally, in an amorphous political system characterized by a lack of group and party activity, a mayor is denied both information and support. By examining the ways in which the performance of Oakland's mayor diverges from a model of political leadership, we can identify some of the factors that support or limit such leadership.

By JEFFREY L. PRESSMAN, Instructor in Government, Dartmouth College.

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525 **Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the Imbalance Thesis.** This research note analyzes a key premise of the thesis of "institutional imbalance" in less developed nations, the idea that administrative modernity promotes overparticipation in the performance of political and governmental functions. Judgmental data on fifty-seven Latin American, Asian, and African nations suggest that level of administrative development is highly correlated with overparticipation, but in precisely the opposite direction from that predicted in the literature—that is, the relationship is negative. This finding indicates the necessity of reconsidering some of the conventional wisdom of comparative administration.

By LEE SIGELMAN, Instructor in Political Science, Vanderbilt University.

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529 **The Permanent Court of International Justice, the International Court of Justice, the League of Nations and the United Nations: A Comparative Empirical Survey.** After a brief discussion of the existing literature and a description of the data set, the paper compares the role of four international institutions in two-party disputes in terms of (1) participant behavior and characteristics,

(2) relationships between participants, and (3) characteristics of the disputes and institutional responses. The major purpose of the comparison is to assess various middle-range theoretical ideas that have been suggested by scholars and to determine whether the differences on the variables are a function of structural (PCIJ and ICJ versus League and UN) or historical (PCIJ and League versus ICJ and UN) variations. A final section of the paper presents a scheme for further research on the role of international institutions in the international bargaining process.

By William D. Coplin, Director, International Relations Program and Associate Professor of Political Science and J. Martin Rochester, Research Associate, International Relations Program, Syracuse University.

551 **The Swing Ratio and Game Theory.** We propose a simple game-theory model of single-member plurality electoral systems, two parties with unequal resources being the players. Strategies consist of allocations of resources among the *n* contests, and a party's payoff is the number of contests to which it has assigned more resources than the other party. Mixed strategies exist which are asymptotically optimal as *n* increases. Identifying a party's proportion of total resources with its total vote proportion, we predict that the swing ratio, or marginal seat proportion per vote proportion, is 2. This compares to empirical findings which range between 2 and 4, and to the hitherto unexplained cube law, which predicts 3. We suggest that the strategic problem modeled by this game accounts for the major part of the swing ratio effect. Factors which vary from system to system, such as proportion of hard-core support attached to parties, may amplify this effect.

By David Sankoff, Visiting Member at the Centre de recherches mathématiques, Université de Montréal and Koula Mellos, Lecturer, Département de science politique, Université d'Ottawa.

555 **The Strategy of Ambiguity: Uncertainty and Electoral Competition.** In this paper problems of social choice in general, and political choice in particular, are considered in light of uncertainty. The space of social alternatives in this formulation includes not only pure social states, but lotteries or probability distributions over those states as well. In the context of candidate strategy selection in a spatial model of political choice, candidate strategy sets are represented by pure strategies—points in the space of alternatives—and ambiguous strategies—lotteries over those points. Questions about optimal strategy choice and the equilibrium properties of these choices are then entertained. Duncan Black's theorem about the dominance of the median preference is generalized, and further contingencies in which the theorem is false are specified. The substantive foci of these results are: (1) the conditions in which seekers of political office will rationally choose to appear equivocal in their policy intentions; and (2) the role of institutional structure in defining equilibrium.

By Kenneth A. Shepsle, Assistant Professor of Political Science, Washington University, St. Louis.

569 **Cluster-bloc Analysis and Statistical Inference.** Cluster-bloc analysis is a useful method of examining the voting records of a legislature, in order to find what subgroups of members regularly vote together. Agreement scores are calculated for every legislator with every other legislator. Then when a group is found to have all its members in high agreement with each other they are referred to as a cluster bloc. These groups, which are discovered empirically, are not necessarily the same as the formal caucus groups. So far each researcher has had to use his own judgment as to what constitutes "high agreement," but it can be shown that the cutoff points can be established statistically, against the null hypothesis of random voting. Since each score can be tested for significance, it is possible to use statistically based indices of cohesion for the legislature or any specified subgroup and indices of adhesion between the various subgroups. Examples are given for the African group in the UN General Assembly.

By Peter Willetts, Research Associate, Department of Political Science and Public Administration, Makerere University, Kampala.

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By Charles O. Jones, Maurice Falk Professor of Politics, University of Pittsburgh.

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From Confusion to Clarity: Issues and American Voters, 1956–1968*

GERALD M. POMPER
Livingston College, Rutgers University

Students of politics, from Plato to Marcuse, have frequently sneered at the inability of the "masses" to discern political reality. Whether citizens are misled by the shadows on the wall of the cave or by the shadows of the television tube, they are deemed to be fundamentally unequipped consciously to control political elites and rationally to direct public policy.

In contemporary times, such conclusions have been unintentionally strengthened by selected and often distorted findings of empirical voting research. In broad terms, political scientists have found voters to have limited interest in politics, to be strongly attached to their traditional parties and social groups, and to lack ideological coherence in their views of political issues.[1] Of particular concern here is the electorate's perception of issues, of coherent ideologies, and of the links between issue preferences and partisan preferences. Much voting research has indicated that these perceptions are cloudy. Large proportions of voters have "no opinion" or "don't know" their opinion on specific policy issues.[2] Only 12 per cent of the citizenry has been found to hold an ideological view of the parties.[3] The links between issue preferences and party choice are weak. Party identification was found in *The American Voter* to have little relation to general ideology,[4] and McClosky, dealing with the same period, found "that substantial differences of opinion exist among the electorate on only five of the 24 issues" he examined.[5] The Michigan volume indicated that there was little belief among the electorate that the parties differed on particular issues, and little agreement on the direction of whatever differences were perceived.[6] Although the findings were carefully qualified, their general thrust justified the conclusion: "The electoral decision gives great freedom to those who must frame the policies of government. If the election returns offer little guidance on specific policies, neither do they generate pressures that restrict the scope of President and Congress in developing public policy."[7]

Reaction to these conclusions has varied. Some publicists or would-be "hidden persuaders" have selectively drawn from the voting studies to compose a picture of an irrational voter, easily manipulated by possessors of this arcane knowledge. Thus, a widely quoted book on "the New Politics" is taken seriously when it warns, "These new managers . . . can play upon the voters like virtuosos. They can push a pedal here, strike a chord there, and presumably, they can get precisely the response they seek."[8] To Bernard Berelson, the voting findings offered the occasion to develop a new theory of democracy, in which stability depended on the limited involvement of the population.[9]

V. O. Key saw the danger that the findings of voting research could be misused to convert popular democracy into a system of elitist manipulation, in which, "fed a steady diet of buncombe, the people may come to expect and to

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* Mr. Thomas O'Donnell deserves many thanks for his work in arranging and computing the data on which this paper is based. I also appreciate the many suggestions of my colleagues at Rutgers University particularly Benjamin Barber, W. Carey McWilliams, Stephen Salmore, and Gordon Schochet, and the comments of John Kessel and Rick Piltz.

[1] The most important past works are, chronologically: Paul Lazarsfeld, Bernard Berelson, and Helen Gaudet, *The People's Choice,* 2nd ed. (New York: Columbia University Press, 1948); Bernard Berelson, Paul Lazarsfeld and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Angus Campbell, Gerald Gurin and Warren Miller, *The Voter Decides* (Evanston: Row, Peterson, 1954); Eugene Burdick and Arthur Brodbeck, eds., *American Voting Behavior* (New York: The Free Press, 1959); Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: Wiley, 1960), Ithiel de Sola Pool, Robert Abelson and Samuel Popkin, *Candidates, Issues and Strategies* (Cambridge: MIT. Press, 1964); Philip Converse, "The Nature of Belief Systems in Mass Publics," in David Apter, ed. *Ideology and Discontent* (New York: The Free Press, 1964); and Campbell, Converse, Miller and Stokes, *Elections and the Political Order* (New York: Wiley, 1966). Important recent works will be cited below.

[2] V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1961), Chap. 4; Campbell et al., *The American Voter,* p. 174.

[3] Campbell et al., *The American Voter,* p. 249.

[4] Campbell et al., *The American Voter,* chap. 9.

[5] Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," *American Political Science Review,* 54 (June, 1960), p. 419. Among party leaders, by contrast, differences existed on 23 of 24 issues.

[6] Campbell et al., *The American Voter,* pp. 182–184.

[7] Campbell et al., *The American Voter,* p. 544.

[8] James M. Perry, *The New Politics* (New York: Clarkson Potter, 1968), p. 213.

[9] Berelson, Chap. 14.

respond with highest predictability to buncombe."[10] In his last work, he sought to revive the notion that issue and voting preferences are closely related. While he did demonstrate a certain consistency in the electorate's choices, he could not prove a causal connection. Key's work, along with Arthur Goldberg's,[11] however, did bring a renewed emphasis on the analysis of policy preferences to electoral studies.

In the past few years, this renewed emphasis has been evident in a number of independent reappraisals of *The American Voter*'s finding of ideological unawareness among the electorate. One set of authors argues that mass ideological awareness has always been present to a greater degree than found by Campbell, Converse, Miller and Stokes, but that appropriate methods have not been employed to observe this awareness. Thus, focusing on local concerns, Luttbeg found considerable "constraint" or coherence, in mass attitudes.[12] Similarly, using a series of prepared statements, Brown found no difference in ideological awareness between political articulates and inarticulates.[13] Most notable is the work of Lane, who was able to discern a developed ideology among New Haven workers through lengthy interviews.[14] These strands suggest that previous studies demonstrated not the absence of ideology, but the absence of the ability to articulate hidden ideology.[15]

Another criticism is that the findings of *The American Voter* are time-bound. They may show a low degree of ideology among voters in 1956, but only because the 1956 election did not stimulate ideological feelings. Replicating the Michigan study for the contrasting 1964 election, in which ideology was emphasized, Field and Anderson found a substantial increase in ideological awareness. In the Goldwater-Johnson context, a third of the respondents are classified as ideological, more than double the proportion of 1956 voters, and nearly three times the proportion of the total sample in the earlier study.[16]

The recent work of Pierce combines these two revisions. Using three measures of ideological awareness derived from Survey Research Center materials, instead of only one, Pierce also tested the change in awareness over time. A greater proportion of the sample was classified as ideological under the three-pronged analysis, and the proportion was found to have increased considerably from 1956 through 1960 to 1964.[17] These recent findings suggest an increased awareness of politics among American voters. The analysis below provides further evidence to support this conclusion.

Changing Perceptions of Parties and Policies

The research presented here does not deal with general ideology, but with voter opinions and perceptions on the six precise policy questions which have been asked consistently of the national Survey Research Center sample in each quadrennial study since 1956. I will analyze the relationship between issue preferences and three partisan variables: party identification, the awareness of differences between the parties on these six issues, and the consensus among the electorate on the positions of the parties on these issues. In each of the three instances, we will find an increase in voter consciousness during the 1956–1968 period.

The six policy issues are: federal aid to education, government provision of medical care, government guarantee of full employment, federal enforcement of fair employment and fair housing, federal enforcement of school integration, and foreign aid. Although the questions are not worded identically in the four surveys, they are sufficiently close to be highly comparable. Moreover, we are not concerned with the trend of opinion itself, but rather with comparisons of the structure of opinion in each of the election years, so that identical wording is not critical.[18]

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[10] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), p. 7.

[11] Arthur Goldberg, "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review*, 60 (December, 1966), 913–922; "Social Determinism and Rationality as Bases of Party Identification," *American Political Science Review*, 63 (March, 1969), 5–25.

[12] Norman Luttbeg, "The Structure of Beliefs among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 398–409.

[13] Steven R. Brown, "Consistency and the Persistence of Ideology," *Public Opinion Quarterly*, 34 (Spring, 1970), 60–68.

[14] Robert Lane, *Political Ideology* (New York: The Free Press, 1960).

[15] See John Plamenatz, "Electoral Studies and Democratic Theory: I. A British View," *Political Studies*, 6 (February, 1958), 9.

> A choice is reasonable, not because the chooser, when challenged, can give a satisfactory explanation of why he made it but because, if he could give an explanation, it would be satisfactory. The reasoning that lies behind the choice is often made in private language which the chooser never learns to translate into words intelligible to others because there is ordinarily no need for him to do so.

[16] J. O. Field and R. E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly*, 33 (Fall, 1969), 380–398. While the coding in this research differs somewhat from that of *The American Voter*, the changes were essentially those made necessary by new procedures of the Survey Research Center.

[17] John G. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970), 25–42. Confirming evidence is found in the recent work of David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400. Using responses to open-ended questions, RePass finds considerable mass concern for issues, an increase in issue awareness from 1960 to 1964, a close relationship between issue position and partisanship, and a significant partial correlation of .23 in 1964 between issue partisanship and vote, controlling for candidate image and party identification.

In Table 1, positions on these issues are presented along a five-point scale of party identification. The statistic reported is the percentage taking the "liberal" position (i.e., in favor of federal government action). For 1956, the data clearly support the contention of *The American Voter* that issue preferences were essentially unrelated to party identification. A linear relationship between the two variables existed only on the issue of medical care. The situation changed substantially, however, over the next twelve years. By 1968, a linear relationship existed essentially on all issues but foreign aid. The change was not gradual, but became suddenly apparent in the election of 1964. When John Kennedy was elected President, the relationship of opinion and party identification was hardly different from that of the Eisenhower period. By 1964, however, linear relationships were evident on four of the six issues, and they began to appear on the other two as well.

The same pattern is evident in the increasing spread between the extremes. Strong Demo-

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[18] The differences in wording consisted largely of changes in form. In 1956 and 1960, the questions were asked as statements with which the respondent could agree or disagree (and also indicate the intensity of his opinion), e.g., "If cities and towns around the country need help to build more schools, the government in Washington ought to give them the money they need." In 1964 and in 1968, the respondent was offered a choice between two policies, each of which was advocated by "some people," such as—"the government in Washington should help towns and cities provide education," or "this should be handled by the states and local communities." There are two differences in wording of possible substance. In 1956 and 1960, the question dealing with full employment asks whether or not "the government in Washington ought to see to it that everybody who wants to work has a job and a good standard of living," while the later alternative does not include "who wants to work." In the earlier surveys, the question on racial equality asks whether, "If Negroes are not getting fair treatment in jobs and housing, the government should see to it that they do," while in later years the question is more narrowly presented as dealing with jobs alone. I believe the basic thrust of these questions is not affected by these changes. The questions used are, by deck and column numbers, in 1956: 3/12, 3/18, 3/21, 3/24, 3/33, 3/54; in 1960: 4/59, 4/67, 4/55, 4/63, 4/72, 4/61; in 1964: 4/45, 4/56, 4/61, 5/11, 5/14, 4/67; in 1968: 4/54, 4/58, 4/60, 4/74, 4/76, 5/29. The surveys of 1948 and 1952 could not be used because questions were insufficiently comparable.

Table 1. Party Identification and Policy Position, 1956, 1960, 1964 and 1968
(In Percentages Supporting "Liberal" Position)

Party Identification	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
Strong Democrat	80.0	66.8	51.0	53.6	74.2	74.5	78.2	81.3	75.6	71.2	52.6	53.1
Weak Democrat	78.1	59.0	44.1	38.3	67.3	60.2	65.2	72.1	64.0	62.4	38.4	39.7
Independent	71.0	53.2	39.3	32.9	55.8	56.7	57.2	55.3	55.0	56.6	31.0	27.0
Weak Republican	68.7	39.1	21.5	22.5	51.4	47.5	43.5	39.3	59.5	43.9	25.9	24.9
Strong Republican	67.7	44.5	15.5	12.0	45.9	54.2	23.6	42.7	51.5	52.7	16.1	25.4
Gamma	.15	.20	.34	.36	.24	.18	.45	.41	.19	.16	.31	.25

Party Identification	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
Strong Democrat	73.3	63.0	56.3	61.9	38.7	39.8	53.7	58.9	49.5	51.4	64.7	51.3
Weak Democrat	71.3	63.1	42.9	43.5	44.4	37.5	43.2	44.6	55.4	48.8	59.2	45.8
Independent	66.6	65.4	50.3	37.7	48.8	47.1	49.0	37.3	49.9	53.2	57.5	42.7
Weak Republican	70.8	62.7	36.3	37.8	49.3	43.0	50.5	37.4	48.2	54.0	56.6	47.0
Strong Republican	66.8	65.9	20.6	31.3	38.8	41.5	34.8	31.5	51.4	61.5	49.7	41.8
Gamma	.04	−.02	.22	.24	.04	−.01	.08	.43	.01	−.03	.08	.04

Table 2. Perceptions of Parties on Policy Issues, by Party Identification

Group	Existence of Party Differences											
	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of Those with Opinions Perceiving Party Differences)											
Strong Democrat	58.1	62.8	73.8	75.7	63.2	68.6	86.4	85.8	73.0	74.5	80.3	78.1
Weak Democrat	44.2	53.2	60.9	53.0	48.9	59.0	77.2	71.2	54.2	66.3	69.3	65.4
Independent	43.4	52.3	53.5	48.0	41.2	45.3	77.4	62.3	36.4	57.6	55.2	54.2
Weak Republican	48.0	41.8	58.9	53.3	50.0	68.6	75.3	65.8	50.3	50.5	61.3	64.1
Strong Republican	59.0	63.0	78.4	73.3	64.9	66.7	87.7	75.9	63.7	42.5	76.7	79.1
Total Sample	52	55	66	59	54	58	82	71	55	64	70	63

Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of Those with Opinions Perceiving Party Differences)											
Strong Democrat	50.0	52.5	77.5	78.5	55.6	50.6	70.0	75.9	46.5	44.7	61.5	64.7
Weak Democrat	46.9	38.7	70.1	64.9	55.6	34.9	63.7	62.5	40.4	37.5	52.7	43.8
Independent	38.5	38.7	58.8	55.0	42.3	30.8	59.0	51.1	33.6	32.7	50.8	39.3
Weak Republican	46.0	44.3	53.2	47.7	50.0	36.4	52.1	53.1	50.0	32.0	50.3	42.8
Strong Republican	52.3	50.0	75.0	60.5	57.5	39.2	66.2	60.5	63.9	47.4	61.8	48.0
Total Sample	47	44	69	62	51	36	64	60	49	39	56	46

crats and Strong Republicans were already distinct on the issue of medical care in 1956, but the difference almost doubled in 1964 and remained considerably widened in 1968. The same pattern is evident on the other issues as well. Party differences became quite high by 1964 and then decreased slightly (on job guarantees, fair employment, and foreign aid) or increased slightly (on aid to education and school integration) four years later.

The increased policy distinctiveness of partisans is summarized by the increases in the ordinal correlation (*gamma*) included in the last row of Table 1. Policy and party preferences became far more congruent in 1964, and the correlation was very similar in 1968. The only major exception is the issue of school integration. For the first three elections in this period, no meaningful correlation existed between party identification and support of federal action on the issue (*gammas* = .04, −.01, .08). But by 1968, there was a considerable relationship, and the *gamma* statistic (.43) is higher than in any other instance. On five of the six issues—all but foreign aid—party identification meant something by 1968 other than a traditional reaffirmation: it was now related to the policy preferences of the voter.

A second change is evident in the awareness of party differences on these six policy questions. In 1956, the Survey Research Center found little awareness of differences. It subjected its respondents to a series of questions on each policy question. It eliminated those who had no interest in, or opinion on, an issue, as well as those who, holding an opinion on the issue, could not decide if "the government is going too far, doing less than it should, or what?" This latter question typically eliminated more respondents than the query on the issue itself. Those who were left after the multiple screening were then asked whether there was a difference between the parties. This remaining sample was presumably a relatively knowledgeable group, but in 1956 even *they* found rather little distinction between the parties on most issues.[19] In later surveys, the question on the government's program was eliminated. Therefore, the remaining sample asked to differentiate the parties was larger, and presumably less informed. Nevertheless, they perceived more of a difference between the parties.

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[19] The answers of 1956 respondents can be found in Table 8-3 of Campbell et al., *The American Voter*, p. 182. It might be argued that 1956 voters were far more issue-conscious than the data reveal, but that they were thinking about different issues than those raised by the parties or the survey. This argument seems hardly plausible, since it would require a degree of ideological originality for which there is no evidence among any mass public.

This changing awareness is seen in Table 2. Over time, the parties have come to be seen as more different on questions of federal government power. Again, the issue of foreign aid is exceptional. The critical effect of the 1964 election is evident once more, as the proportions seeing party differences changed most dramatically in the Goldwater-Johnson contest. In 1968, there was a regression downward toward lessened perceptions of party splits, although it was not a full regression. Only on the distinct issue of foreign aid was the perception of party difference in 1968 at or below the level of the Eisenhower period. For the other issues, during the decade of the 1960s, a significant and apparently enduring political lesson was learned about the existence of party differences. The lesson was particularly well learned by the stronger partisans. On virtually all issues and in all elections, the strong partisans were more likely to see party differences than were weak partisans, who in turn were more likely to see differences than independents. These results clearly accord with the concept and significance of party identification.

A more important finding is involved in the third change, relating to the ideological identity of the parties. In 1956, even among the sample remaining after various filterings, there was relatively little consensus on the position of the parties. The greatest consensus existed on another question dealing with ownership of electric power and housing. On this item, three-fourths of those who saw a party difference also agreed that the Republicans were more favorable to private ownership of these industries.[20] The identification of the parties' ideological positions was lower on the six issues we are considering. (The item on power and housing was not used in later surveys, so it cannot be analyzed here.)

By 1968, as seen in Table 3, each of the two parties seemed to have a much clearer identity. The proportion seeing the Democrats as the more liberal or activist party had risen on every question, even on foreign aid. It is particularly significant that in almost all cases the perceptions of the parties showed a consensus greater

Table 3. Consensus on Positions of Parties on Policy Issues, by Party Identification

Group	Consensus on Party Positions											
	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)											
Strong Democrat	90.5	95.2	96.5	94.1	93.9	95.8	98.2	98.8	85.5	98.8	98.8	97.0
Weak Democrat	86.5	90.5	93.6	82.6	84.6	90.4	95.8	88.7	71.8	93.3	93.9	88.5
Independent	66.3	74.6	83.8	64.3	76.4	87.7	93.5	82.7	61.9	82.3	89.3	71.4
Weak Republican	37.2	60.9	55.8	58.7	64.3	52.2	77.6	85.0	60.0	67.3	72.5	69.4
Strong Republican	31.6	47.9	55.0	41.2	57.8	51.3	79.8	63.6	47.0	32.3	63.8	53.9
Total Sample	67	76	81	73	77	81	92	87	69	80	88	80

Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)											
Strong Democrat	64.6	83.5	96.9	97.3	39.1	34.7	96.2	95.3	57.1	80.9	98.1	93.0
Weak Democrat	63.6	65.5	93.9	88.3	51.5	42.5	90.3	87.1	53.0	60.2	94.2	89.2
Independent	49.4	53.4	89.5	75.6	45.8	46.4	91.8	83.4	54.5	46.8	88.1	83.1
Weak Republican	23.6	18.3	81.0	70.2	45.1	49.3	83.9	75.4	46.2	13.0	67.8	81.5
Strong Republican	6.0	17.2	67.6	56.4	59.4	48.5	71.3	69.5	29.3	21.4	70.8	61.6
Total Sample	48	52	89	83	48	44	89	86	43	49	88	85

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[20] Campbell et al., *The American Voter,* p. 182.

than that which existed on the exceptional issue of ownership of industry in the Eisenhower period. By 1968, in other words, to judge by six important items, the majority of the electorate had become more aware of party differences and had come to agree that the Democratic party was the liberal party.[21]

The change in perceptions of the parties' positions is not affected by controls for party identification. In all partisan groups, the Democrats were increasingly recognized as the liberal party on each of the six issues. Democrats were most likely to make this judgment, but by 1964 a majority even of Strong Republicans recognized the liberal credentials of the opposition. In 1956, by contrast, the Strong Republicans conceded this "honor" to the Democrats on only two issues.

The acceptance of the liberalism of the Democrats is most evident in the 1964 survey, conducted during the contest of Goldwater and Johnson. It was not solely related to that election, however, for important changes in perceptions of party position on the economic issues of aid to education, medical care, and job guarantees can be located as early as the 1960 campaign. The identification of the Democratic party as the liberal faction largely persisted through the 1968 election as well, although there was some lessening of perceived party positions, most notably on the issue of federal aid to education.

The most striking change occurred on racial issues. In 1956, there was no consensus on the parties' stands on the issues of school integration and fair employment. Differences between the parties were less likely to be seen, and Republicans were as likely as Democrats to be perceived as favoring federal action on civil rights. A startling reversal occurred in 1964: all partisan groups recognized the existence of a difference on this issue, and all were convinced that the Democrats stood more for government programs on behalf of blacks. Even Strong Republicans conceded the point they argued in the Eisenhower years. The identification of the Democrats with civil rights was slightly attenuated in 1968, but it still was a clearer perception than existed on any other issue.

Sources of Change

What accounts for these shifts? The increasing correspondence of party identification with policy preferences is probably related to the new perceptions of the parties as holding relatively distinctive and identifiable positions on these issues. Generational change is one possible explanation of these new perceptions. Younger, more ideologically attuned voters may have replaced older and less sensitive electors. To test this possibility, age is controlled in Table 4. Respondents in the age groupings used in the 1956 survey are compared to cohorts four, eight, and twelve years older in 1960, 1964, and 1968, respectively. If generational replacement is the main reason for the changed perceptions, the greatest differences would be seen between the upper-right and lower-left segments of each set of percentages. If political aging is not an explanation, changes would be most evident among cohort groups.[22]

The data show that both conclusions are valid to some extent but that generational turnover is not an adequate explanation in itself. In virtually all comparisons, the new generation (below age 23 in 1964 or below age 27 in 1968) was more aware of party differences and more perceptive of Democratic liberalism than those it replaced (those 65 or older in 1956, or 69 or older in 1960, who were too depleted by 1964 and 1968 to be included). These differences are not as large, however, as are the changes of matched cohorts. Some political learning occurred in all age groups. Thus, in 1956 and 1960, most age groups did not see a party difference on racial issues, and all of them tended to identify the Republicans as more liberal. Particularly in 1964 and even in 1968, all age groups came to perceive a party difference, and there was intergenerational agreement on the Democratic party's greater support of fair employment and school integration.

The data give some indication of greater growth in awareness among older, rather than younger voters. The cohort aged 25–34 in 1956 (37 to 46 in 1968) appears to have become particularly aware of Democratic liberalism in 1964 and to have held to its new perceptions in 1968, Speculating on the source of this change, we could note that this group came of age politically just after the realignment of the New Deal. Hence this group may not have been immediately affected by the political events of that period but may have been particularly susceptible to the effect of similar issues in 1964.

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[21] The perceptions of party are asked in different ways in the four surveys, so the data must be handled differently. In 1956, respondents were asked which party "is closer to what you want." To locate those who believe the Democrats are liberal on federal aid to education, for example, one must combine those who favor the policy, and think the Democrats are closer to their own position, with those who oppose the policy, and think the Republicans are closer. In 1960, 1964 and 1968, the question was asked in a straightforward manner, which party is likely to favor federal aid to education. These data are located, in 1956: two columns to the right of the policy question; in 1960: in decks and columns 4/60, 4/70, 4/56, 4/64, 4/75, 4/62; in 1964: 4/45, 4/60, 4/63, 5/13, 5/18, 4/69; in 1968: immediately after the policy question, except for party stands on school integration, 4/79.

[22] On the method of cohort analysis, see Neal E. Cutler, "Generation, Maturation and Party Affiliation: A Cohort Analysis," *Public Opinion Quarterly*, 33 (Winter, 1969–70), 583–592.

Degree of educational achievement might be an alternative explanation. The average level of schooling has increased considerably in the United States in this twelve-year period. Greater perception might result from the intellectual upgrading of the population, with less informed grade school or high school graduates being replaced by informed college alumni. Controlling for education would reveal this effect by indicating relatively little change by educational level. This control is presented in Table 5, which clearly shows that educational upgrading does not explain the shift in perceptions. Awareness has increased at all educational levels.

In fact, the disparities in perceptions among persons of varying schooling have tended to lessen, particularly in regard to identifying the Democrats as the liberal party. This effect was most marked in the 1964 campaign; in that year, on every issue, respondents with only a grade school education were clearer in their perceptions of the parties than college graduates were in 1956. The educational gap widened slightly in 1968, but the level of awareness remained substantially higher in all educational strata. In one sense, therefore, the political events of this period provided a tax-free learning substitute for the political education that might otherwise have occurred in high school and college classrooms. (The vocational implications for political science faculty members might best be left aside.)

A final control may be made for region and race, as in Table 6, since the changed perceptions noted above may have been concentrated in particular segments of the population. Such differences are evident in Table 6, although the changes are also apparent in both the North and South, and among whites as well as blacks. Southern whites particularly stand out. Their perceptions varied widely over the course of the four elections, but the net result has been relatively little change in their awareness of party differences or in their ideological identifications of the parties. They changed almost exclusively in their identification of the Democrats as the more liberal party on issues of civil rights. These changing perceptions may well be related to the growth of Republican voting in the white South.[23]

Blacks' opinions also varied widely, but the net result has been a substantially increased awareness of party differences and a clear identification of the Democrats as the liberal party. While this change is most dramatically evident on the civil rights issues, and among southern blacks, it is not confined to these issues or to the former Confederacy. Negroes of both regions also became more cognizant of differences between the parties and of Democratic liberalism on economic issues. It is interesting to note, moreover, that within each region in all years, blacks tended to be more conscious of political differences than whites. Given their perceptions of increasing differences between the parties, it is not surprising that blacks now overwhelmingly identify with and vote for the Democrats.[24]

There is no obvious demographic cause for the changed awareness of party differences by the electorate. Neither the passing of generations, nor improved education, nor regional and racial variations provides a simple explanation. The fact remains that, during these twelve years, all segments of the population displayed considerable political learning (or misperception, depending on one's view of the "real" character of the parties). The alternative to a demographic explanation is a directly political one: the events and campaigns of the 1960s, I suggest, made politics more relevant and more dramatic to the mass electorate. In the process, party differences were developed and perceived. Democrats divided from Republicans, Democrats became more liberal, and voters became more aware.

There are many correspondences between the events of the political world and the voters' perceptions of the parties. In the 1960 campaign, John Kennedy and Richard Nixon, and the Democratic and Republican platforms, dif-

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[23] See: Bernard Cosman, "Republicanism in the South," *Southwestern Social Science Quarterly*, 48 (June, 1967), 13–23; Philip Converse, Warren Miller, Jerrold Rusk and Arthur Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), esp. 1095–1101. The Wallace Campaign in 1968 did not substantially affect the perceptions of the major parties' positions on civil rights. In noting the parties' stands, a respondent could answer that there was no difference between the major parties, but Wallace did represent a distinctive position. Few respondents chose this option; those who did were included in this analysis with the "no difference" group.

[24] Of the 1968 SRC sample, only 3 of the 149 blacks identified themselves as Republicans, and only 3 voted for Nixon.

Table 4. Perceptions of Parties on Policy Issues, by Age

Existence of Party Differences

Group		Aid to Education				Medical Care			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(Percentages of Those with Opinions Perceiving Party Differences)							
——	Under 27	—	—	60.0	52.2	—	—	84.7	64.6
Under 25	27–36	49.1	67.2	58.4	56.7	57.8	74.0	83.9	68.7
25–34	37–46	49.1	45.7	65.3	59.8	49.0	51.4	82.5	77.8
35–44	47–56	45.2	53.1	72.7	62.9	50.2	66.1	81.0	68.6
45–54	57–66	53.3	58.0	65.9	59.6	54.0	52.0	76.6	71.4
55–64	Over 66	52.2	58.8	55.2	56.5	59.0	58.0	78.1	74.9
65 and over	——	54.0	59.3	—	—	53.5	52.6	—	—

Group		Job Guarantee				Fair Employment			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(Percentages of Those with Opinions Perceiving Party Differences)							
——	Under 27	—	—	65.8	65.8	—	—	78.1	60.5
Under 25	27–36	46.6	68.7	66.0	61.5	50.7	57.3	69.9	64.3
25–34	37–46	55.3	66.3	73.0	70.8	51.5	37.8	68.1	63.3
35–44	47–56	49.5	66.0	68.2	74.6	44.1	39.0	66.3	65.0
45–54	57–66	58.0	62.3	71.8	62.2	50.2	48.5	66.8	54.0
55–64	Over 66	63.9	59.0	66.2	70.3	39.1	42.6	65.6	58.2
65 and over	——	58.8	54.5	—	—	52.7	46.7	—	—

Group		School Integration				Foreign Aid			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(Percentages of Those with Opinions Perceiving Party Differences)							
——	Under 27	—	—	75.6	56.2	—	—	54.5	42.2
Under 25	27–36	58.2	41.1	67.1	59.6	42.5	42.3	55.1	39.2
25–34	37–46	52.7	38.4	61.6	65.4	48.8	38.9	60.6	49.3
35–44	47–56	50.2	46.6	64.9	57.7	40.4	37.4	55.0	47.5
45–54	57–66	49.3	40.6	61.9	61.3	44.5	37.8	55.6	47.2
55–64	Over 66	53.1	28.5	54.7	56.9	44.9	33.0	54.8	50.6
65 and over	——	50.0	34.4	—	—	47.2	46.6	—	—

fered on federal aid to education[25]—and the voters became more likely to regard the Democrats' position on the issue as the more liberal. Medicare became a major partisan issue during the Kennedy administration, resulting in a series of partisan Congressional roll calls on the issue, and a platform conflict during the 1964 campaign. Perceiving this disagreement, the voters were much more likely in 1964 to see a difference between the parties on the issue of medical care and to identify the Democrats as more favorable to governmental action on the issue. When a Republican administration acted vigorously to promote school integration, as in Little Rock, the voters tended to believe that the G.O.P. was more favorable to this policy. This opinion was particularly common in 1956 and 1960 among Southerners and blacks. When a Democratic administration came to support new civil rights legislation, when it sent marshals and National Guardsmen to desegregate the University of Alabama and the University of Mississippi, when it reacted sympathetically to the protests of Martin Luther King in Birmingham and Selma, the voters drew the conclusion that there was a difference between

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[25] Kirk Porter and Donald Johnson, *National Party Platforms,* 3rd ed. (Urbana: University of Illinois Press, 1966), pp. 590–614.

Table 4—(*Continued*)

Consensus on Party Positions

Group		Aid to Education				Medical Care			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(*Percentages of Those Perceiving Differences Selecting Democrats as Liberal*)							
———	Under 27	—	—	89.9	65.6	—	—	96.7	86.0
Under 25	27–36	51.8	77.9	86.2	72.3	73.8	82.4	92.2	88.6
25–34	37–46	66.0	73.5	84.8	79.4	78.9	89.4	94.1	91.5
35–44	47–56	75.8	74.6	77.4	67.0	81.6	80.0	87.5	87.6
45–54	57–66	69.7	90.2	86.5	73.1	80.2	82.7	95.5	83.3
55–64	Over 66	67.1	72.6	74.0	73.1	72.0	71.3	90.6	80.8
65 and over	———	51.8	60.8	—	—	64.5	75.9	—	—

Group		Job Guarantee				Fair Employment			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(*Percentages of Those Perceiving Differences Selecting Democrats as Liberal*)							
———	Under 27	—	—	92.0	79.0	—	—	93.6	83.7
Under 25	27–36	76.0	82.6	92.7	78.3	54.7	55.4	93.0	83.0
25–34	37–46	65.0	82.4	88.5	85.9	43.6	52.5	88.3	83.5
35–44	47–56	72.2	81.6	85.4	66.7	42.7	45.3	85.8	84.7
45–54	57–66	65.8	85.2	89.0	83.9	53.0	63.1	91.5	82.0
55–64	Over 66	62.5	72.1	90.8	74.5	48.0	42.0	89.3	75.6
65 and over	———	71.4	64.9	—	—	47.7	56.2	—	—

Group		School Integration				Foreign Aid			
Age in '56	Age in '68	'56	'60	'64	'68	'56	'60	'64	'68
		(*Percentages of Those Perceiving Differences Selecting Democrats as Liberal*)							
———	Under 27	—	—	91.2	85.5	—	—	87.5	85.2
Under 25	27–36	50.0	34.9	92.6	84.5	34.8	50.0	90.6	85.8
25–34	37–46	42.6	44.6	92.0	90.0	60.1	44.0	88.1	87.7
35–44	47–56	46.8	40.0	85.8	85.3	40.6	46.0	86.5	85.9
45–54	57–66	51.0	50.0	90.0	84.6	48.1	60.1	88.0	83.2
55–64	Over 66	57.2	35.2	86.9	79.6	47.3	50.0	83.7	80.5
65 and over	———	49.0	52.3	—	—	38.6	46.0	—	—

the parties and identified the Democrats as more favorable to the cause of the Negroes. When the Republican party adopted a "Southern strategy" in 1968 and its candidate largely ignored the issue of civil rights, the voters continued to hold this position and clearly to distinguish the parties.[26]

The most important electoral event of this period appears to be the 1964 Presidential campaign. Senator Barry Goldwater consciously sought to clarify and widen the ideological differences between the parties. The evidence presented here indicates that he accomplished his goal, although it did not benefit the Republican party. Voters, previously unable to see differences between the parties, learned the lesson of "a choice, not an echo." They accepted the Senator's characterization of the Republicans as conservative and the Democrats as liberal, and, on the specific issues involved, they preferred the liberal alternative.[27] This process of education was not confined to the insightful young, or to the formally-trained college popu-

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[26] On the development of medicare, civil rights and other programs in this period, see James L. Sundquist, *Politics and Policy* (Washington: Brookings Institution, 1968).

[27] Also see Lloyd Free and Hadley Cantril, *The Political Beliefs of Americans* (New Brunswick: Rutgers University Press, 1967), Chap. 2.

Table 5. Perceptions of Parties on Policy Issues, by Education

Existence of Party Differences

Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of those With Opinions Perceiving Party Differences)											
Elementary School	45.7	51.8	61.6	56.1	49.2	53.4	76.0	70.7	59.9	65.3	68.8	63.7
High School	52.2	55.4	61.6	53.0	50.0	60.0	80.6	68.0	52.3	71.2	65.5	61.4
Some College	49.0	58.2	66.9	61.7	57.0	63.8	84.3	68.9	48.1	56.4	71.2	68.1
College Graduate	65.2	60.2	73.2	67.4	61.7	63.9	87.8	84.1	55.2	56.5	71.9	74.9

Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of those With Opinions Perceiving Party Differences)											
Elementary School	47.2	45.9	68.2	62.5	50.0	35.2	63.3	61.9	40.2	39.0	51.0	46.3
High School	49.1	36.3	67.7	61.2	54.1	37.3	61.3	57.1	43.1	35.5	54.8	47.5
Some College	46.5	48.3	67.5	63.1	51.7	34.4	66.7	60.2	47.1	42.3	55.7	42.2
College Graduate	44.3	44.4	65.8	57.6	51.8	41.2	58.9	61.7	62.0	36.5	68.3	51.6

Consensus on Party Positions

Group	Aid to Education				Medical Care				Job Guarantee			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of those Perceiving Difference Selecting Democrats as Liberal)											
Elementary School	65.7	71.8	86.4	75.0	70.9	75.3	92.7	84.0	68.8	78.0	90.1	82.0
High School	62.3	73.9	77.6	70.3	75.5	85.0	90.3	87.0	69.9	83.6	87.2	76.2
Some College	67.5	78.9	77.4	71.6	82.9	81.1	89.4	86.4	65.0	80.0	85.6	79.2
College Graduate	75.8	88.7	86.9	75.8	89.0	93.9	95.4	92.0	72.2	82.9	90.0	84.4

Group	Fair Employment				School Integration				Foreign Aid			
	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of those Perceiving Difference Selecting Democrats as Liberal)											
Elementary School	49.7	53.7	91.6	79.7	45.5	51.5	87.2	84.8	46.5	47.8	88.4	84.8
High School	53.0	63.8	88.5	81.3	44.5	41.7	91.2	83.3	40.6	58.2	86.0	83.5
Some College	39.2	44.0	85.3	83.6	51.0	36.7	87.0	87.7	51.0	40.5	83.6	84.3
College Graduate	46.5	39.5	91.3	88.9	56.0	35.8	96.6	87.0	56.0	54.8	97.2	86.4

lation, or to committed White segregationists and Black integrationists. This political education was general and apparently persistent. The party characteristics which had been so clearly marked in 1964 remained relatively evident to the voters four years later. The lessons remained learned even though differences between the candidates had narrowed considerably, and although some of the previous issues, such as aid to education and medical care, had been partially resolved. Differences were not perceived as clearly in 1968 as in 1964, but they were far more apparent to the voters than in the Eisenhower years.

Implications of the Data

The data developed in this article lead to three implications. First, the central importance of the 1964 campaign lends support to the supposition that this election was a critical election, initiating a new political era in the United States, rather than the aberrant event it appeared at the time. A critical election, such as that of the New Deal, is one in which a

Table 6. Perceptions of Parties on Policy Issues, by Region and Race

	Existence of Party Differences											
	Aid to Education				Medical Care				Job Guarantee			
Group	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of Those With Opinions Perceiving Party Differences)											
North White	48.0	57.3	61.1	58.0	51.2	53.6	81.1	71.5	52.4	63.2	66.4	65.2
North Black	66.7	80.0	71.8	72.6	61.5	57.9	83.3	79.2	75.0	79.0	78.8	77.0
South White	51.0	49.3	68.8	50.2	54.6	57.6	78.3	65.3	61.5	62.2	69.8	61.0
South Black	62.4	50.8	80.8	87.7	54.7	45.7	83.3	88.1	64.5	68.3	81.8	90.9
	Fair Employment				School Integration				Foreign Aid			
Group	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentages of Those With Opinions Perceiving Party Differences)											
North White	43.2	44.2	63.5	59.2	49.5	37.3	59.2	57.6	43.3	36.2	53.2	45.0
North Black	79.5	73.2	88.2	87.5	57.5	57.2	85.7	81.2	43.5	50.9	57.8	45.1
South White	52.7	36.4	67.4	56.1	57.0	30.6	64.3	56.0	49.7	39.1	57.9	44.9
South Black	70.0	60.0	89.3	89.9	58.5	46.0	82.2	88.1	61.8	38.1	63.3	75.0
	Consensus on Party Positions											
	Aid to Education				Medical Care				Job Guarantee			
Group	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)											
North White	66.4	75.9	81.2	68.0	74.8	81.7	93.1	87.0	66.0	78.8	88.8	79.0
North Black	77.2	84.4	100.0	95.0	81.2	91.0	97.5	100.0	76.0	91.1	97.3	96.0
South White	63.8	80.8	74.3	70.0	81.9	79.3	83.6	79.0	67.4	82.5	81.3	72.4
South Black	75.0	53.3	98.4	100.0	82.3	71.4	98.7	98.2	80.0	86.1	98.6	100.0
	Fair Employment				School Integration				Foreign Aid			
Group	'56	'60	'64	'68	'56	'60	'64	'68	'56	'60	'64	'68
	(Percentage of Those Perceiving Difference Selecting Democrats as Liberal)											
North White	47.1	78.8	88.8	81.1	54.2	48.3	91.2	84.0	45.2	52.2	89.5	84.1
North Black	65.7	83.7	98.0	100.0	60.8	32.1	100.0	100.0	70.0	57.1	84.6	86.9
South White	34.8	48.0	79.0	75.0	34.4	37.4	77.5	79.6	45.7	41.4	83.2	82.4
South Black	60.6	53.8	100.0	100.0	35.3	17.6	100.0	100.0	61.5	43.8	100.0	100.0

deep and enduring cleavage in the electorate becomes evident.[28] Characteristic of such elections is increased voter consciousness of policy questions, and the later electoral persistence of group divisions based on the policy questions raised in the critical election.[29] These hallmarks of a critical period are evident in the upsurge of mass perceptions of party differences in 1964 and the persistence of these perceptions in 1968. While the voters did not respond ideologically in the full sense of the term, they did respond to the specific issues presented to them, and they did align their partisan loyalties far closer to their policy preferences. Such readjustments occur rarely, but they are the decisive moments in American political history.

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[28] V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), pp. 3–18. The concept and its significance have been deeply researched in Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970).

[29] The point is elaborated well in Everett C. Ladd, *American Political Parties* (New York: Norton, 1970), pp. 1–10, and illustrated historically in the body of this book.

Research at the time of the 1964 election led to the conclusion the electorate did not respond primarily in ideological terms,[30] although there already were signs available that deeper forces were moving the voters.[31] Subsequent work by younger scholars, perhaps less committed to prevailing commonplaces, has shown the growth of ideological constraints and divergent "party images" during the turbulent decade of the 1960s.[32] Moreover, the most current controversies are tending to emphasize the ideological division between the parties. Partisan conflicts on such issues as national health insurance, federal revenue sharing, or enforcement of equal employment opportunities are likely to reinforce the popular belief that Democrats favor increased federal government action and Republicans prefer private or state governmental action.[33] Perceptions of party differences are therefore likely to be confirmed and extended.

Confirming evidence is needed to demonstrate a critical election in 1964. Such evidence would include shifts in party identification among substantial groups of voters and the emergence of new issues. The data presented here do not speak to these questions. Indeed, they deal only with issues already evident in 1956. We do find, however, increased coherence of policy positions within each party following, and new sharp cleavages associated with racial issues. These findings are congruent with those expected in a period of realignment.

Clarification and realignment of the parties' policy positions leads to a second implication, the possible development of a "responsible two-party system" in the United States. Such a system was advocated by a committee of the American Political Science Association in 1950.[34] That committee, however, expected more of voters than was possible at the time. To effectuate the system of party government advocated in the APSA *Report*, the voter must perceive a relationship between his policy preferences and his partisan choices. Furthermore, for the parties to serve as links between voter preferences and public policies, their programs must be perceived as somewhat distinct and reasonably clear. These are necessary but not sufficient conditions for developing a responsible party system in which parties put forth programs, receive popular approval of these programs, and then carry out the popular mandate.

That these conditions did not exist at the time of the APSA *Report* is indicated by our data for the 1956 election. On most issues, the voters did not relate their policy preferences to their partisan affiliations nor did they see a difference between the parties, nor did they agree on the relative positions of the two parties. In recent years, however, the situation has changed. There is a sizeable statistical correlation now between party identification and policy preference on the particular issues studied. Voters now do see a difference between Democrats and Republicans and agree on their ideological character.

Parties can now meaningfully stand as "groups of like-minded men" offering particular stances toward public issues. Their victories in elections can now reasonably be interpreted as related to the mass choice of one set of issue positions over another. To this extent, the conditions for a responsible party system have been fulfilled.

Other necessary conditions, however, have not been met. Greater voter awareness of party differences does not in itself create a mandating election system. It would also be necessary to show that votes are actually cast *because of* issue preferences. Moreover, a responsible party system requires not only a responsible electorate to provide direction, approval, or castigation for the parties. It also requires coherent, effective, and relatively disciplined parties. In fact, both in the APSA *Report* and in the British party system which its authors cited with admiration, party government is crucially dependent on the internal discipline of the parties, even more than on the electorate.[35] There is comparatively little evidence of the development of cohesive parties in the United States.[36] Many developments in fact have tended in the opposite direction, such as the diffusion of party power to wealthy individuals, media specialists, and campaign managers; the challenge to entrenched party leaders through party primaries and the decentralizing reforms of national conventions; and the separation of national, state, and local elections. The electorate is more ready today than in the 1950s for a responsible party system, but the parties may be too weakened for the task.

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[30] See Philip Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 330–335.

[31] See: Walter Dean Burnham, "American Voting Behavior and the 1964 Election," *Midwest Journal of Political Science*, 12 (February, 1968), 1–40; John Kessel, *The Goldwater Coalition* (Indianapolis: Bobbs Merrill, 1968), pp. 301–308; Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), Chap. 5; David Segal, "Partisan Realignment in the United States: The Lesson of the 1964 Election," *Public Opinion Quarterly*, 32 (Fall, 1968), 441–444; RePass, "Issue Salience and Party Choice," 398–400.

[32] See: David Nexon, "Hacks, Fanatics, and Responsible but Dense Voters," (Unpublished M.A. Thesis, University of Chicago, 1970); and Rick S. Piltz, "Mass support for the Political Parties: Bases for Realignment," (Ph.D. Dissertation, University of Michigan, in progress).

[33] See the roll calls listed in *Congressional Quarterly Weekly Report*, 29 (January 29, 1971), 220–222, dealing with the 91st Congress.

[34] American Political Science Association, Committee on Political Parties, "Toward a More Responsible Two-Party System," *American Political Science Review*, 44 (September, 1950), Supplement.

[35] On the British electorate, see David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1969), esp. Part IV.

The final implication of the data relates to the study of American voting behavior. Because of the excellence with which the Michigan studies have been conducted and presented, we have tended to overgeneralize the findings of such studies as *The American Voter.* We have assumed that this superb analysis of the 1950s is a study of the electorate of all time. Because voters of the Eisenhower period did not respond to the parties in ideological terms, we have often concluded that they could not respond in such terms. Yet, Key reminded us that "the voice of the people is but an echo ... The people's verdict can be no more than a selective reflection from among the alternatives and outlooks presented to them."[37] If the parties do not emphasize issues, or do not present distinct and clear positions, the voters are unlikely to invent party programs. When there *are* party positions and differences, the voters can perceive them. Students of electoral behavior must be sensitive to these changing stimuli and reactions.

In observing electoral behavior and mass ideology, we particularly must beware of our own ideological biases. It is instructive to remember that *The American Voter* was written at the time of "the great American celebration," when the dominant academic ideology was that the United States had reached the "end of ideology" through a consensual agreement on twentieth-century liberalism.[38] It was therefore comforting and appropriate to find that ideology was absent among the mass public. In the absence of ideology, basic social change was difficult to imagine. The existing political system was therefore seen as stable and relatively insulated from change. Throughout the Michigan studies, the emphasis is on the inertial elements of American politics, an emphasis that, in keeping with the tone of the era, plays down the possibility and desirability of basic social change.[39]

In the last decade, after a series of political shocks, ideological conflict has been resurrected, and consensus has been severely disrupted, if not destroyed. Basic issues have been raised again, and the electorate has shown itself able to comprehend and respond to such conflicts. Furthermore, an emphasis on ideological passivity is itself seen to be ideological. Earlier findings on the electorate remind us of the dangers of overgeneralization from data derived from a limited temporal or spatial context.

More generally, we must remember to be aware of the political context of voting behavior. Perhaps the major fault of the Michigan studies has been the comparative neglect of the political environment as an independent variable. The methodology of survey research has brought an overemphasis on the individual behavior of isolated respondents. The influences upon these respondents have been studied only indirectly, through the voters' personal perceptions and actions.[40] But voters in fact are not isolated, for they are affected by their environment,[41] the mass media, the economic system, and the prevailing ideology. More attention must be devoted to these shaping influences.

Most critically, we must emphasize in this context the effect upon voters of the stimuli they receive from the parties and other electoral actors. If these stimuli are issueless and static, as they largely were in the 1950s, the citizenry is likely to respond in the manner described in *The American Voter.* If these stimuli are more ideological and dynamic, we are likely to see different perceptions and behavior, such as that evidenced in the 1964 and 1968 elections.[42] "In the large the electorate behaves about as rationally and responsibly as we should expect, given the clarity of the alternatives presented to it and the character of the information available to it."[43] Confused voters reflect confused parties; clarity among the voters follows from clear-headed parties.

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[36] Sundquist, however, in *Politics and Policy,* Chaps. 9 and 12, argues that Democratic party actions in the 1950s and 1960s already constituted the creation of a responsible party system. In contrast, see Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in *Elections and the Political Order,* Chap. 11. Originally published in 1962, the latter work shows the absence of conditions for responsible parties, at least before the possibly critical election of 1964.

[37] Key, *The Responsible Electorate,* p. 2.

[38] Daniel Bell, *The End of Ideology* (New York: The Free Press, 1960); Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace and World, 1955).

[39] For example, note the concluding chapter of *The American Voter* or Chaps. 2, 8, 10, 12 of *Elections and the Political Order,* which are largely reprints of earlier articles.

[40] Note the criticisms of Kenneth Prewitt and Norman Nie, in "Revisiting the Election Studies of the Survey Research Center," a paper prepared for delivery at the 1970 meeting of the American Political Science Association, p. 18:

> The SRC group has written persuasively regarding the implications for American politics of the findings about citizen information and awareness. They have less critically discussed the implications for voter rationality of their findings about election processes and alternatives.

[41] Warren Miller recognized this point in "One Party Politics and the Voter," *American Political Science Review,* 50 (Sept., 1956), 707–725.

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[42] The recent work of the Survey Research Center has given more emphasis to dynamic elements. See Donald Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966), 19–28; and Converse et al., "Continuity and Change in American Politics."

[43] Key, *The Responsible Electorate*, p. 7.

Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election*

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This paper presents two arguments. One argument is that the 1968 election bears witness to the importance of issues at critical points in political history. American surveys show that issues vary greatly over time in their total impact on elections. From 1936 to 1948 the question of the role of the federal government had a major effect on the flow of voters between the parties.[1] In contrast, studies conducted during the rather apolitical period of the 1950s demonstrated both a lack of ideological structure in people's beliefs and a marginal impact of issues in influencing their votes.[2] With the Goldwater and Wallace insurgencies of 1964 and 1968, issues became again important components of electoral decisions.[3] This was particularly true of Vietnam, urban unrest and race, social welfare, and Johnson's performance as president—the dominant issues of 1968.

A second argument is that issues having a similar impact on the election outcome can have very different consequences for popular control of public policy. On some issues the electorate exercises no effective constraints on leaders' policy choices. On others, the electorate permits political leaders a wide array of options when a policy is adopted but passes a retrospective judgment on the policy in subsequent elections. On still other issues, the public severely limits the options of leaders at the time policy is made. Race and public order, it is argued, are examples of issues on which the electorate places the most significant restraints on the policy initiatives of political leaders.

A Normal Vote Analysis of Issues in 1968

Converse's normal vote technique is a particularly useful means of relating issues to the vote.[4] Essentially, the method provides an esti-

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* Fred I. Greenstein, Herbert H. Hyman, and J. Donald Moon of Wesleyan University, C. Richard Hofstetter of Ohio State University, and Robert D. McClure of Syracuse University criticized an earlier draft of this paper. Charles S. Hyneman of Indiana University first stimulated my interest in popular control of public policy; his books and his conversation have formed my ideas on the subject. Without committing these people to a defense of the product, I would like to acknowledge and thank them. The data utilized in this study were made available by the Inter-University Consortium for Political Research. The data were originally collected by the Survey Research Center of the University of Michigan. Neither the Center nor the Consortium bear any responsibility for the analyses or interpretations presented here.

[1] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), Chap. 3.

[2] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), Chaps. 8–10. Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David Apter, (New York: Free Press, 1964), pp. 206–61. Philip E. Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue," in Edward R. Tufte, ed., *The Quantitative Analysis of Social Problems* (Reading, Mass.: Addison-Wesley, 1970), pp. 168–89. Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406–27.

[3] For analyses of issues in the 1964 election, see Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966), 19–28; John H. Kessel, *The Goldwater Coalition* (Indianapolis: Bobbs-Merrill, 1968), Chap. 9; Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 321–36; Walter Dean Burnham, "American Voting Behavior and the 1964 Election," *Midwest Journal of Political Science*, 12 (Feb., 1968), 1–40; and David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400. For 1968 see Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (Dec., 1969), 1083–105; Herbert F. Weisburg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review* 64 (Dec., 1970), 1167–85; Seymour Martin Lipset and Earl Raab, "The Wallace Whitelash," *Transaction*, 7 (Dec., 1969), 23–35; Samuel A. Kirkpatrick and Melvin E. Jones, "Vote Direction and Issue Cleavage in 1968," *Social Science Quarterly*, 51 (Dec., 1970), 689–705; and Gerald M. Pomper, "Controls and Influence in American Elections (Even 1968)," *American Behavioral Scientist*, 13 (Nov./Dec., 1969), 215–30.

[4] Philip E. Converse, "The Concept of a Normal Vote," in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: Wiley, 1966), pp. 9–39. Aside from this seminal article, which utilizes the normal vote to estimate the importance of the religious issue in 1960, the normal vote technique has seen limited application. Two exceptions are J. Merrill Shanks, "The Impact of Voters' Political Information on Electoral Change: A Reexamination of the Quality of American Electoral Decisions," (unpublished Ph.D. dissertation, University of Michigan, 1970), which presents an extension of Converse's work; and John H. Kessel, *Goldwater Coalition*, p. 294–97. Two other

mate of a party's *expected* proportion of the vote for any selected subgroup of the electorate in a normal election. This estimate is based on three factors: the balance of party identifiers within the group, the typical defection rates of identifiers to other parties, and the turnout rate. In the American context, a normal vote is defined as one in which the defection rates of Republicans and Democrats to the opposing parties are equal, with an adjustment made in the prediction in recognition of the greater tendency of Republicans to vote and to vote loyally for their party's candidate.[5]

Applying the concept of a normal vote to a political issue requires a calculation of normal (expected) votes for each group of persons holding different positions on the issue. Computing these expected votes is quite straightforward:[6]

(1) Responses to the issue are cross-classified against the Survey Research Center measure of party identification.

(2) The scores (+2, +1, 0, −1, −2) are assigned to the five classes of identifiers (Strong Democrat, Weak Democrat, Independent, Weak Republican, Strong Republican).

(3) From the scores, a mean party identification score is calculated for the distribution of party identifiers within each response category of the issue item.

(4) The obtained mean (M) is then inserted into the prediction equation,

$$V = .483 + .268M,$$

where V is the expected Democratic proportion of the vote. This linear equation is an approximation of a more complex equation. Converse states the linear equation estimates the predictions of the more complex equation within +1 to −1 per cent for means within the range of +.8 to −.8. This confidence interval includes the range of expected Democratic vote percentages from 27.1 to 69.5.

The intuitive meaning of the normal vote equation is easily grasped. The constant (48.3 per cent) is the expected Democratic vote for a group of identifiers that is perfectly balanced among Republicans and Democrats. (Thus, the mean [M] would be zero, and the predicted Democratic vote [V] would equal the constant.) The fact that the Republicans would win a majority from a group equally divided between Republicans and Democrats reflects the disadvantage the Democratic party suffers as a result of the greater relative loyalty and turnout of Republican partisans. A predominantly Democratic population yields a positive mean which adds to the constant, just as a Republican population yields a negative mean which diminishes it.

As the equation makes clear, the only factor that enters directly into the calculation of an expected vote for a group of people is the distribution of party identifiers within it. This equation, while cheap in data costs, does presume that the expected Democratic vote is the same for all subgroups of the electorate having the same distribution of party identifiers. This assumption is not as questionable as it might first appear. Shanks has begun an extension of Converse's procedures, adding to the prediction equation a more theoretically pleasing set of variables. Since Shanks's estimates of the expected Democratic vote for several subgroups are almost identical to those of Converse for the presidential elections of 1952–1964, I have used Converse's more parsimonious equation.[7]

The utility of the normal vote calculation is that it permits the analyst to separate issues into their *partisan* (long-term) components and their *short-term* components. Consider Figure 1 as an example of a normal vote analysis of an

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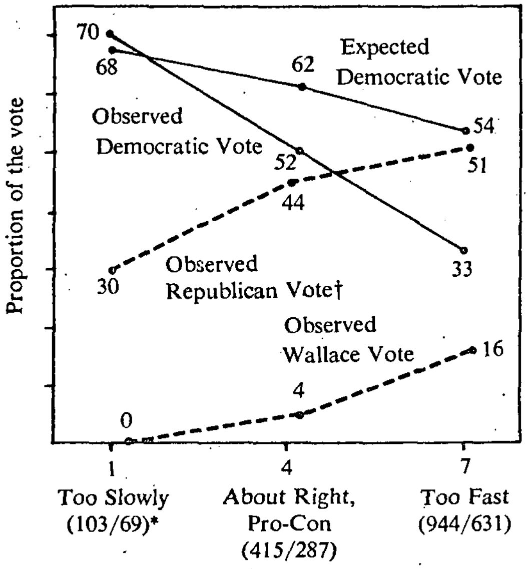
articles have used the intuitive notion of the normal vote, though the authors estimated the normal vote with aggregate data. See Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science*, 23 (Feb., 1969), 58–83; and Barbara Hinckley, "Incumbency and the Presidential Vote in Senate Elections: Defining the Parameters of Subpresidential Voting," *American Political Science Review*, 64 (Sept., 1970), 836–42.

[5] Though Republicans vote at higher rates than Democrats, the effect of the *total* turnout rate on the two-party vote division is slight. Converse, "The Concept of a Normal Vote," pp. 28–30. Kabaker, "Estimating the Normal Vote . . . ," pp. 78 and 83. Therefore, the assumption that Converse must make regarding the level of turnout underlying the normal party division is not critical.

[6] Converse, "The Concept of a Normal Vote," pp. 34–39. A normal vote analysis for a particular issue requires only simple arithmetic and two cross-classification tables (one to compute the expected party vote and another to determine the actual party vote). Therefore, the technique is particularly suitable for undergraduate research exercises.

[7] Shanks, "Impact of Voters' Political Information . . . ," p. 149. Converse and Shanks define a normal vote differently. Converse defines a normal vote empirically, by averaging the results of several elections and by assuming that short-term forces are averaged out in the process. Shanks treats a normal election as a pure, hypothetical concept and obtains estimates of the propensity of individuals to vote for a party by a technique of simulation. Shanks's results are strongly affected by the constraints he places upon the simulation (a fact he is well aware of). An important consideration in my use of Converse's equations is that one of Shanks's constraints is that the simulation must produce results comparable to those of Converse.

Figure 1. The Normal Vote and Speed of Movement on Civil Rights



Item: "Some say that the civil rights people have been trying to push too fast. Others feel they haven't pushed fast enough. How about you: Do you think that civil rights leaders are trying to push too fast, are going too slowly, or are they moving about the right speed?"

* In the parentheses, the first figure is the frequency base for the computation of the expected Democratic vote. It includes all party identifiers and independents. The second figure is the frequency base for the computation of the actual vote percentages. It includes all respondents who cast votes for one of the three major candidates.

† The expected Republican vote is 100 minus the expected Democratic vote.

important issue in 1968—the speed with which civil rights leaders have been pushing their cause.

The Partisan Component of a Civil Rights Issue. *The partisan component is the degree of party polarization on an issue.*[8] Party polarization in turn is a function of two factors:

(1) One aspect of polarization is the degree of association of issue positions with party identification. Party identification is both stable over the lifetime of most voters and a major determinant of their electoral choices.[9] Therefore, an issue on which mass party members are polarized sinks its roots into a stable, long-term division in the electorate. Knowing the balance of party loyalties of people holding some view on a particular issue, one can predict how these persons would vote in a normal election. These predictions are called *expected Democratic votes.*

The partisan component of an issue is manifest in changes in the expected Democratic proportion of the vote across issue positions—the greater the changes, the more partisan are people's attitudes on the issue. In Figure 1 the balance of party identification among persons believing that civil rights people have been pushing too slowly so favors the Democrats that the Democrats would expect to receive 68 per cent of the votes in a normal election. In contrast, among those who believe civil rights people are pushing too fast, the Democrats would normally expect to receive 54 per cent of the vote. This difference of 14 percentage points (68 minus 54) reflects the relative degree of polarization of the mass parties on the issue.

(2) A second aspect of polarization is the proportion of people advocating each of the issue positions. Polarization is born of conflict of opinions; as consensus is approached, an issue ceases to divide the parties. In Figure 1, nine times as many people think civil rights people are pushing too fast as think they are pushing too slowly. In Stokes's terms this is almost a valence issue, an issue on which there is no disagreement;[10] as such, it cannot be a partisan issue.

The Short-Term Component of a Civil Rights Issue. *The short-term component of an issue is its relationship to defections from the normal party vote.* In Figure 1 the short-term component is distinct. Humphrey received more than his full complement of expected votes from those who thought civil rights people were pushing too slowly. But those who believed civil rights leaders were pushing too fast gave Humphrey only 33 per cent of their votes, 21 percentage points below what Humphrey normally could have expected. The greatest proportion of the losses in this latter group went to Wallace. The expected Republican vote for this group is 100 minus the expected Democratic vote of 54 per cent, or 46 per cent. Nixon actually received 51 per cent of their votes, which was 5 points better than he could normally expect. The remainder

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[8] The careful reader will note that I am defining the concept of a partisan component differently than Converse does. In my usage the partisan component of an issue is simply its relation to party identification. Converse defines "partisan forces" as short-term stimuli, which are pro-Democratic or pro-Republican in varying degrees of strength and which induce defections from the normal party vote. Converse, "The Concept of a Normal Vote," p. 15.

[9] Campbell et al., *The American Voter,* Chaps. 6–7.

[10] Donald E. Stokes, "Spatial Models of Party Competition," in Campbell et al., *Elections and the Political Order,* p. 170. The opposite of a valence issue is a position issue.

(21 minus 5) of Humphrey's 21 point loss is the 16 per cent Wallace vote in that category. Again, we note the distribution of opinion on the issue. The rate of defections of Democrats from Humphrey was greatest among those who believed civil rights leaders were pushing too fast, a group which easily held the majority position on this issue.

What can we conclude from the observation of the deviations from the normal Democratic vote? Certainly not that the issue was causally related to the vote or that the issue made some net contribution to the observed Democratic vote. The reason is that from survey data it is difficult to determine with precision the issues (if any) that lead each person to cast his vote for a particular party. Lacking a method of categorizing the electorate, normal vote analysis cannot ascertain the net gain or loss of an issue to a candidate.

What we can say is this: For respondents in this sample the distribution of party identification is such that the Democrats would expect to receive 57 per cent of a normal vote.[11] In fact Humphrey received 41 per cent, 16 percentage points below a normal Democratic vote. If Humphrey had lost this same 16 percentage points across every issue position, then this issue would not even be statistically related to short-term defections from the normal vote. Such is not the case with the civil rights issue in the previous example. Humphrey exceeded his expected vote among some civil rights groups and fell short in others. The variation of the actual gains and losses in each issue category (+2, −10, and −21) from the overall deficit (16 points) is a measure of the issue's strength of association with short-term defections from the normal vote. Thus, even if we cannot speak in causal terms of an issue's contribution to the vote, we can compare issues statistically in terms of their association with long-term party loyalties and short-term defections from the normal party vote. Appendix II provides the formulas for such comparisons.

With this explanation of normal vote analysis, we can now decompose a set of 15 issues in the 1968 SRC election study and compare them in terms of their long- and short-term components. This set of issues falls into four broad groupings: Vietnam, social welfare, urban unrest and race, and civil liberties and public protest. A final issue orientation, the electorate's evaluation of Johnson's performance as president, does not fit neatly into any of these categories; it will be considered separately.

Vietnam: The Vulnerability of the Status Quo Position. The 1968 election study includes two very useful items probing attitudes toward the Vietnam war: escalation vs. de-escalation, and immediate withdrawal vs. military victory. Both reveal similar patterns, illustrated by Figure 2, a presentation of preferences for escalation vs. de-escalation.

As one would predict on a matter of foreign policy, there are no long-term, partisan aspects of people's attitudes about Vietnam.[12] Republicans and Democrats are almost equally likely to endorse solutions ranging from pulling out of Vietnam entirely to taking a stronger stand even if it means invading North Vietnam. This is reflected in the similarity of the expected Democratic votes in the three categories of policy alternatives.[13]

In contrast, the short-term component of at-

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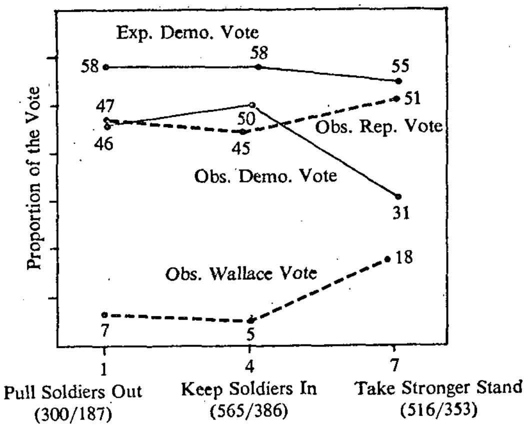
[11] On the basis of the five presidential and congressional elections from 1952 to 1960, Converse estimates the normal vote for the whole population to be 54 per cent Democratic. By 1968 the balance of party identification has become slightly more Democratic. Applying Converse's equation, $V = .483 + .268M$, to the 1968 sample gives an expected Democratic vote in 1968 of 57 per cent Democratic. Having recomputed the normal vote for the 1968 sample, I do not have to assume that the distribution of party identification was unchanged from the 1952–1960 period, i.e., that partisan realignment was not taking place in 1968. I am assuming that the probability of an individual party identifier voting for the candidate of his own party remained basically unchanged from the earlier period.

The use of normal vote analysis does present two problems of interpretation. The first problem is that short-term political factors slightly influence responses to the party identification measure. (See footnote 28, Kessel, *Goldwater Coalition*, pp. 295–96.) As a result some short-term political influences will be submerged in the expected party vote. The effect of this result is to make the short-term relationships of issues to votes slightly conservative underestimates of the true relationships. A second, more difficult problem is the Wallace candidacy in 1968. The estimates of the normal vote assume only two major candidates, not three. With Wallace in the race, the normal vote analyses can be interpreted as follows: The normal vote projects the expected vote in 1968 if short-term forces had been in balance and if only two major candidates had run. The Wallace candidacy is then interpretable as a short-term force responsible for deviations from the expected Democratic and Republican votes. In justification of this interpretation, there is scant evidence that many voters identified themselves as members of the American Independent Party of Wallace. On the 1968 party identification measure, only 0.2 per cent of the sample falls into the category reserved for people who think of themselves as members of a minor party.

[12] Campbell et al., *The American Voter,* Chap. 8; V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1964), Chap. 7.

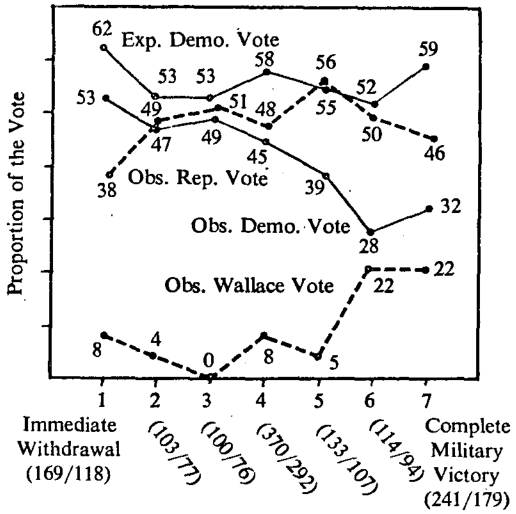
[13] The average of the deviations of the expected Democratic vote in the response categories from the grand expected Democratic vote is only 1.18 per cent. This small average deviation puts this item in last place among the 15 items when ranked according to the magnitude of their partisan components. See Appendix II for a fuller explanation.

Figure 2. The Normal Vote and Escalation/De-escalation



Item: "Which of the following do you think we should do now in Vietnam?"
1. Pull out of Vietnam entirely.
4. Keep our soldiers in Vietnam but try to end the fighting.
7. Take a stronger stand even if it means invading North Vietnam.

Figure 3. The Normal Vote and Immediate Withdrawal/Military Victory



Item: "There is much talk about 'hawks' and 'doves' in connection with Vietnam, and considerable disagreement as to what action the United States should take in Vietnam. Some people think we should do everything necessary to win a *complete military victory,* no matter what results. Some people think we should *withdraw completely* from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale (Show card #4 to R) at point number 1. And suppose the people who support a complete military victory are at the other end of the scale at point number 7. Where would you place yourself [on this scale]?"

titudes on escalation vs. de-escalation is evident. Among those who wanted to pull out entirely, Humphrey's vote is 12 percentage points lower than expected. Among those who would consider invading North Vietnam (34 per cent of the total sample), Humphrey's vote is almost 24 percentage points below its expected level. Humphrey's best showing is among the advocates of a policy of keeping American soldiers in Vietnam while trying to end the fighting. This one might interpret as a status quo position, a position with which the public apparently identified Humphrey because he was a member of an administration pursuing a similar policy.[14]

Obviously, Humphrey fared more poorly among the proponents of escalation than he did among those of de-escalation. His showing was weakest among the advocates of escalation, who outnumbered the advocates of de-escalation by nearly 2:1 among the voters. In addition, the proponents of escalation gave both Nixon and Wallace the greatest proportion of votes they received from any of the other opinion groups (51 and 18 per cent respectively).

The second Vietnam item yields results that are much like those of the first. This item asks the respondents to place themselves on a seven-point scale whose extreme positions are described as "immediate withdrawal" vs. "complete military victory." Figure 3 presents the normal vote breakdown for this issue. The expected or normal Democratic vote across the seven positions varies more or less randomly. As in the previous Vietnam item, the critical component of the issue is its relation to departures from the normal vote. Humphrey sustained his principal losses on the escalation flank; his fewest (49 versus an expected vote of 53 per cent), from a position just left of center. As in the case of the first Vietnam item, the advocates of immediate withdrawal gave Humphrey much more support than did the proponents of military victory. Figure 4 details the pattern of losses on the left and right with a

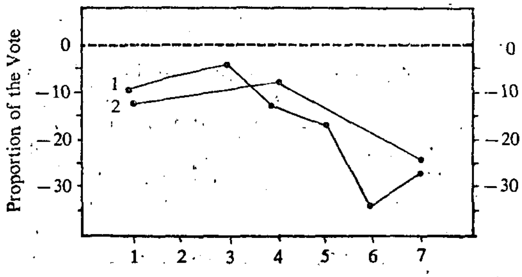
[14] Rosenburg, Verba, and Converse argue, ". . . Vietnam as an issue played little role in deciding the election between Nixon and Humphrey . . . because voters perceived little difference between the candidates." Milton J. Rosenburg, Sidney Verba, and Philip E. Converse, *Vietnam and the Silent Majority* (New York: Harper and Row, 1970), p. 50. Even though the voters saw little difference between the two candidates on Vietnam, the normal vote analysis shows that people at odds with the war policy still punished Humphrey at the polls. For an analysis of the relationship of views on the war to modes of political participation, see Sidney Verba and Richard Brody, "Participation, Policy Preferences, and the War in Vietnam," *Public Opinion Quarterly,* 34 (Fall, 1970), 325–32.

graph of the differences between the expected and the actual Democratic vote on both items.

Urban Unrest and Race: The Future of American Politics. Beliefs concerning urban unrest and race are central to an analysis of the 1968 election; these issues had an unmistakable short-term relationship to the vote, and they appear likely to dominate American politics for years to come. As has often been suggested, it is quite possible that the present Democratic party, based as it is upon a New Deal assemblage of class, ethnic, and regional groups, cannot withstand the impact of these issues on its blue-collar members. The problem of the Democratic party is evidenced in the following analysis.

Figure 1, the example used earlier to explain the concept of a normal vote, illustrates the loss of Democratic support among people who thought that civil rights leaders were pushing their cause too swiftly. The same voting pattern is manifested by people who advocated the use of all available force to quell urban unrest. Figure 5 presents a normal vote analysis of people who were asked to place themselves upon a seven-point continuum concerning alternatives for dealing with the urban problem. One end of the continuum is demarcated by "correct the problems of poverty and unemployment that give rise to the disturbances;" the other, by "use all available force to maintain law and order—no matter what results."[15] As Figure 5 demonstrates, Humphrey captured a substantial majority of the votes of those endorsing a "correct-the-problems" approach. In contrast, he suffered massive losses (34 percentage points) among the advocates of the polar position of force as the solution. Humphrey lost the bulk of these normally Democratic votes to Wallace rather than to Nixon. Nixon pulled his strongest support toward the midrange of the scale, while Wallace's strength peaked at the polar right of the continuum. Among the people of

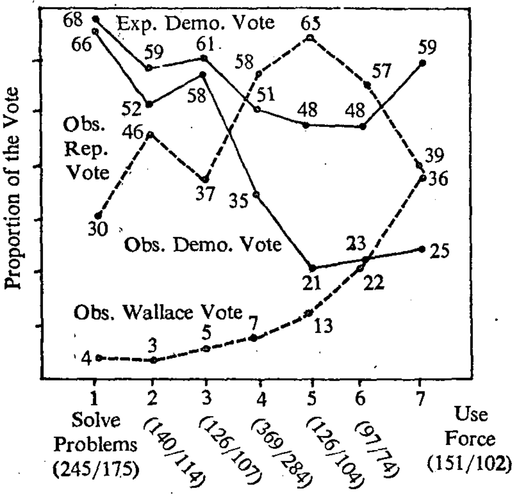
Figure 4. Deviations from the Expected Democratic Vote on Two Vietnam War Items.



Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote. The two lines represent the following items:

1. Escalation vs. de-escalation. See Figure 2 for the item.
2. Immediate withdrawal vs. military victory. See Figure 3 for the item.

Figure 5. The Normal Vote and Urban Unrest



Item: "There is much discussion about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to *use all available force* to maintain law and order—no matter what results. Others say it is more important to *correct the problems* of poverty and unemployment that give rise to the disturbances. And, of course, other people have opinions in between. Suppose the people who stress the use of force are at one end of this scale—at point number 7 (Show card #3 to R). And suppose the people who stress doing more about the problems of poverty and unemployment are at the other end—at point number 1. Where would you place [yourself] on this scale?"

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[15] Two caveats about the item, itself. One is that it poses alternatives that may not stand as such in the minds of the voters. People may distinguish between long- and short-run solutions to riots, advocating use of force as an immediate measure and alleviation of poverty as a longer-run solution. A second difficulty with the item is its penumbra of social welfare content. It clearly presumes that poverty and unemployment are causes of riots and that solving these problems will still them. As anyone knows who has plunged into the ballooning literature on urban violence, competing explanations abound. The gamut ranges from the riff-raff theories of everyman to the exotic aggression theories of the animal behaviorists.

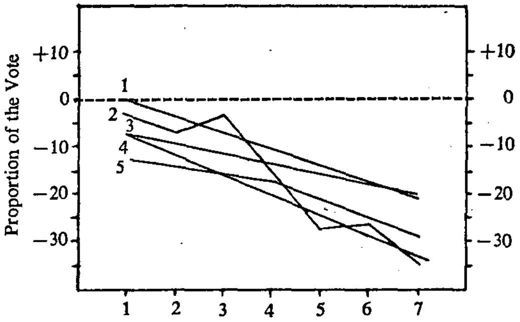
One case for the validity of the item is the pattern of the Wallace vote. The smooth, curvilinear increase in the Wallace vote in Figure 5 indicates that his supporters had no difficulty in interpreting the item. Second, as Figure 6 demonstrates, the vote on this item is quite similar to the vote on the other four items in this set of urban unrest and race issues. Appendix I presents the inter-item associations of this item with others in the race and urban unrest set.

the polar right, Wallace captured so many normally Republican votes that Nixon also fell short of his party's expected strength.

The 1968 SRC election study is rich in items on racial attitudes. In addition to the previous items on speed of movement on civil rights and urban unrest, other questions probe attitudes on segregation in general, attitudes on residential segregation in particular, and perceptions of Negro violence. Since all yield a consistent result under normal vote analysis, Figure 6 presents a composite picture of the vote on racial issues. Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote.

In sum, in an election in which many issues held the public's attention, race and urban attitudes were indisputably important. In competition with a number of salient issues, urban unrest ranks second, belief in segregation ranks fourth, and speed of movement on civil rights ranks sixth in the magnitude of their relationship to defections from the normal vote. Herein may be read the Wallace story. As Converse and others note, in both the North and the South, the Wallace vote came predominantly from Democrats. Nixon rather than Humphrey was the second preference for most of these Democratic Wallace voters. For a third it was at least their second consecutive defection from their national presidential ticket.[16]

Figure 6. Deviations from the Expected Democratic Vote on Five Racial and Urban Unrest Items.



Each line is a plot of the deviation of the expected Democratic vote from the observed Democratic vote. The five lines represent the following items:

1. Moving too fast on civil rights. See Figure 1 for the item.
2. Urban unrest item. See Figure 5 for the item.
3. "During the past year or so, would you say that most of the actions Negroes have taken to get the things they want have been violent, or have most of these actions been peaceful?"
   1. Most have been peaceful. (318/217)
   4. Some violent, some peaceful. (This category is omitted for want of a sufficient number of cases.)
   7. Most have been violent. (1052/694)
4. "What about you: Are you in favor of desegregation, strict segregation, or something in between?"
   1. Desegregation. (539/399)
   4. In between. (689/462)
   7. Segregation. (232/131)
5. "Which of these statements would you agree with?"
   1. Negroes have a right to live wherever they can afford to, just like anybody else. (1042/733)
   4. Don't know; depends; can't decide. (147/94)
   7. White people have a right to keep Negroes out of their neighborhoods if they want to. (339/200)

In the South racial issues dominated all others in 1968. The items on belief in segregation, speed of movement on civil rights, and urban unrest rank first, second, and third, respectively, in their relation to short-term deviations from the normal party vote.[17] Despite that fact, the South also leads the nation in party polarization on racial issues, with Southern Republicans being located farther to the right of Southern Democrats than Northern Republicans are to the right of Northern Democrats.

Civil Liberties and Public Protest. It is instructive to compare the issues of race and civil rights with those of civil liberties and public protest. The protests against the war in Vietnam contributed to the chain of events leading to Johnson's renunciation; the riots in the streets of Chicago during the Democratic convention left the party demoralized.[18] Thus, the protesters may rightly call their effects major ones. But however much the protests may have cost Johnson and his party, they redounded against the protesters as well. Converse et al., and Robinson, underline the antipathy generated by the activities of the demonstrators. Even those who would say that we made a mistake in getting involved in Vietnam and that we ought to pull out immediately supported the actions of the Chicago police. Seventy per cent of this group rejected the suggestion that "too much force" was used by the police; 40 per cent of them would even say that "not enough force" was used to suppress the demonstrators.[19] Wallace, of course, made these protesters the butt of his potent attacks. What then was the relation of these hostile reactions to the vote?

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[16] Converse et al., "Continuity and Change in American Politics," p. 1091. Utilizing sample surveys conducted by the Opinion Research Corporation, Brody and his associates analyzed the effect on evaluations of the candidates in 1968 of urban unrest and immediate withdrawal vs. military victory items identical to those in Figures 3 and 5. Their findings are quite consistent with my own. Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, the Urban Crisis and the 1968 Presidential Election: A Preliminary Analysis," paper delivered at the American Sociological Association Meeting, 1969, pp. 1–32. Other analyses of race and the 1968 elections include Weisburg and Rusk, "Dimensions of Candidate Evaluation"; Seymour Martin Lipset and Earl Raab, "The Wallace Whitelash"; and Arthur G. Wolfe, "Challenge from the Right: The Basis of Voter Support for Wallace in 1968," paper delivered at the American Psychological Association Meeting, Washington, D.C., 1969, pp. 1–20.

[17] The reader might wonder about contaminating interaction effects of sectionalism on many of these issues. Separate normal vote analyses were run for the South and non-South on every item. While the magnitude of the partisan and short-term components differed between the regions somewhat, the basic pattern of the components was remarkably similar on all the issues. That is, issues that were partisan in the non-South were also partisan in the South; defection from the normal vote occurred among the same attitude groups in both regions. This is further evidence for the contention of V. O. Key that, racial attitudes aside, the political atti-

The 1968 election study includes a series of three items concerning the legitimacy of public protests. With respect to each form of protest the respondents are asked to state whether or not they approve its use. The series begins with "protest meetings and marches that are permitted by the local authorities" and ends with "sit-ins, mass meetings, and demonstrations," presuming "all other methods have failed." Figure 7 presents a normal vote analysis of these attitudes.

Surprisingly, the relationship of these issues to the vote is marginal—marginal at least in comparison with others. Of the 15 issues analyzed here, the items for legal meetings and marches, civil disobedience, and mass demonstrations rank respectively twelfth, fourteenth, and fifteenth in the magnitude of their relationships to defections from the normal vote. As one can see from Figure 7, Humphrey fared somewhat better among those who approved of public protest than among those who did not. In the main, however, he lost consistently across all categories of approval and disapproval of protest methods (the gap between the expected Democratic vote [ED] and his observed vote [OD]). The implication of this uniform loss is that other issues were responsible for his defeat.

The failure of civil liberties to surface as a short-term issue in 1968 parallels the fate of internal communism in the early 1950s. In 1952 the Republican party attacked the Democrats with the slogan of "Communism, Corruption and Korea." Yet, Campbell et al. found that only three per cent of the population mentioned in their evaluations of the candidates and parties the argument that the Democratic administration had been "soft on communism."[20] Similarly in a survey conducted in 1954, Stouffer reported that less than one per cent of the population said they were worried about the threat of Communists in the United States.[21] This evidence from surveys is supported by Louis Bean's analyses of aggregate voting returns and Joseph McCarthy's campaigning. Bean concluded that McCarthy's campaigns in 13 states outside the South in 1952 had no effect on the presidential contest and, if anything, hurt the Republican senatorial candidates he had endorsed.[22]

In all likelihood, the reason that these civil liberties and protest items are unrelated to defections from the normal vote is that they have rarely if ever been partisan issues. Democrats are slightly more willing to tolerate these acts than are Republicans. On the whole, however, the public speaks with a single voice against the protests. Not even 10 per cent of the college educated—the principal supporters of tolerance of civil liberties—approve, if asked, of mass demonstrations. Since attitudes on these valence issues are not linked to either parties or candidates, their impact does not extend to the vote itself, though the political system surely feels their force in other ways.

Social Welfare and the Federal Role: The Persistence of Traditional Issues. If the emerging issues of race, the cities, and the war were important to the 1968 election, what of the traditional issues of the post-Depression period? Since the realignment of 1928–36, much of the conflict between the parties has centered upon two broad issues: (1) What is the public responsibility to the private individual for the calamities that can befall him—from economic dislocation, sickness, unemployment, injury, indigence? (2) If there is a public responsibility in these matters, what level of government car-

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tudes of Southerners are very similar to those of people outside the South. Key, *Public Opinion and American Democracy*, pp. 99–105.

[18] Theodore H. White, *The Making of the President 1968* (New York: Atheneum, 1969), Chaps. 4 and 9.

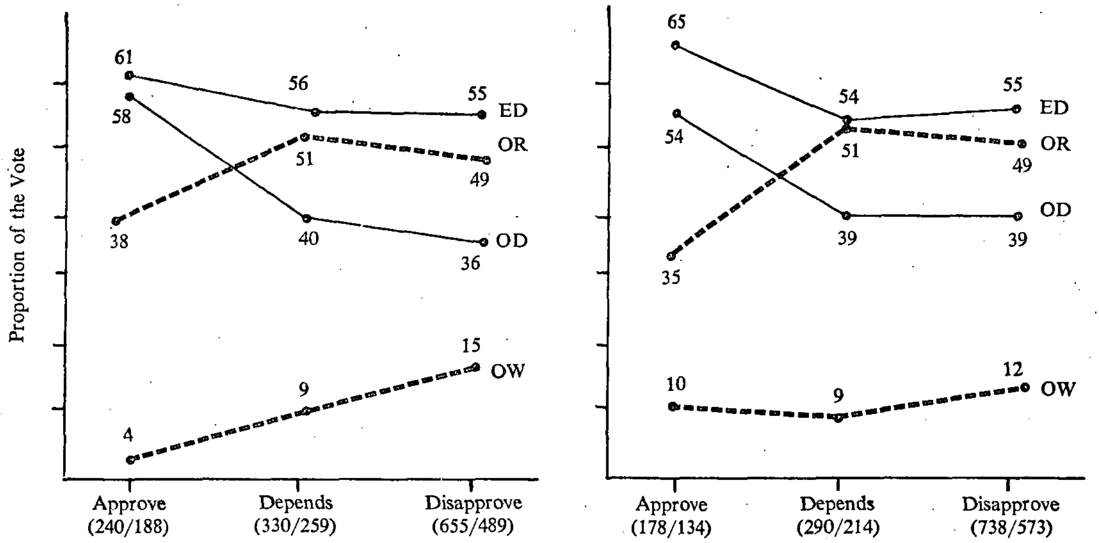
[19] John P. Robinson, "Public Reaction to Political Protest: Chicago 1968," *Public Opinion Quarterly*, 24 (Spring, 1970), 1–9. Converse et al., "Continuity and Change in American Politics," p. 1087.

[20] Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954), p. 52.

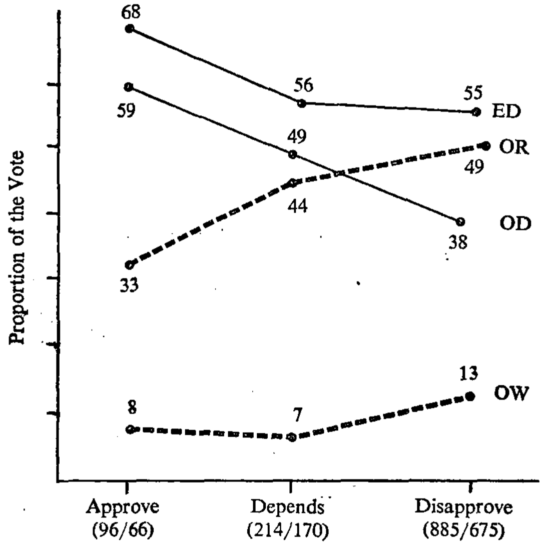
[21] Samuel A. Stouffer, *Communism, Conformity, and Civil Liberties* (Gloucester, Mass.: Peter Smith, 1963), p. 59.

[22] Louis H. Bean, *Influences in the 1954 Mid-Term Elections* (Washington: Public Affairs Institute, 1954), p. 22. This conclusion is supported by Nelson W. Polsby, "Towards an Explanation of McCarthyism," *Political Studies*, 8 (Oct., 1960), 250–71.

Figure 7. The Normal Vote and Civil Liberties Issues


A. LEGAL MEETINGS AND MARCHES

B. CIVIL DISOBEDIENCE


C. MASS DEMONSTRATIONS

Item: "There are many possible ways for people to show their disapproval or disagreement with governmental policies and actions. I am going to describe three ways. We would like to know which ones you approve of as ways of showing dissatisfaction with the government, and which ones you disapprove of."

A. "How about taking part in protest meetings or marches that are permitted by the local authorities?"

B. "How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law?"

C. "Suppose all other methods have failed and the person decides to try to stop the government from going about its usual activities with sit-ins, mass meetings, demonstrations, and things like that?"

ries its burden? The answers of the majority Democratic party have been that there is a proper governmental role in these matters, a role of the national government. Out of this majority have come federal programs such as farm price supports, old age and survivors' insurance, unemployment compensation, and Medicare. The Republican party has, for the most part, resisted the expansion of the federal role at each stage. Because of this continuing conflict, these issues still stand out as the major, long-term issues dividing the parties' adherents.

Figure 8 underlines the long-term, partisan nature of these federal role issues. Again, the measure of the partisan component of an issue is the degree of change in the expected Democratic vote across various positions on the issue (line ED in Figure 8). For the four issues, the difference in the expected Democratic vote between those who would expand the federal government's role and those who would not averages almost 20 percentage points. From a 1958 sample, McClosky observed that the mass membership of the two parties did not differ on issues.[23] This was clearly not the case for federal role issues a decade later.

One might predict the partisan component of these traditional issues; more surprising is their uniformly high relationship to defections from the normal vote. Note, in Figure 8, Humphrey's consistent losses among voters endorsing a limited role for federal activities. In every case the difference between the expected (ED) and the observed Democratic vote (OD) exceeds 20 percentage points. In contrast, Humphrey did quite well among those supporters of a broad scope of federal activities—barely falling short of his expected strength and, in every case, capturing a comfortable majority of the vote. In an election in which race, law and order, and the war loomed on the scene, the more traditional issues seemed reluctant to leave quietly.

The presence of the short-run component of the federal role issues suggests the possibility of spurious association. The conservative positions on these federal role-social welfare issues are associated with four other positions: opposition to civil rights, use of force in dealing with urban unrest, escalation of the war, and a harsh evaluation of Johnson's performance as president.[24] All of the latter issues were highly related to short-term defections from the normal vote in 1968. To check against the possibility that the federal role issues manifest a short-term component only because of their association with the others, separate normal vote analyses of the federal role issues were run within categories of the other issues. Uniformly, the short-term component of the federal role issues remained as strong as in the uncontrolled case.

In sum, the traditional set of attitudes relating to the scope of the federal government persisted as a significant force in the 1968 election. In an election supposedly dominated by urban unrest and the war, why was this so? Perhaps one clue lies in the votes that Humphrey did *not* lose rather than those he did.

In late September, Humphrey's cause looked hopeless. A Gallup poll showed him trailing Nixon by a margin of 43 to 28 per cent and leading Wallace by only seven percentage points. Scarcely a month later, he lost the election by less than one per cent of the popular vote. As Theodore White chronicles the last month of the campaign, Humphrey attacked the Republicans with an echo of the 1964 campaign, the alleged Republican indifference to the traditional issues of social security and social welfare.[25]

Meanwhile, the AFL/CIO had mounted a two-pronged assault on Wallace and Nixon. First, the AFL/CIO carried a message—Wallace and Nixon were no friends of labor. Second, COPE took over much of the effort of mobilizing the electorate, particularly the Black vote. In White's words,

> The dimension of the AFL/CIO effort, unprecedented in American history, can be caught only by its final summary figures; the ultimate registration, by labor's efforts, of 4.6 million voters; the printing and distribution of 55 million pamphlets and leaflets out of Washington and 60 million more from local unions; telephone banks in 638 localities using 8,055 telephones, manned by 24,611 union men and women and their families; some 72,225 house-to-house canvassers; and, on election day, 94,457 volunteers serving as car-poolers, materials-distributors, baby-sitters, poll-watchers, telephoners.[26]

A cross-sectional sample such as this one cannot establish who changed their minds during that last month of the campaign. Yet, the appeals of Humphrey were the traditional ones of the old Democratic coalition. Herein may rest the importance of the short-term effects of social welfare-federal role issues. Humphrey and organized labor used them to counter the new issues—successfully so among those Democrats

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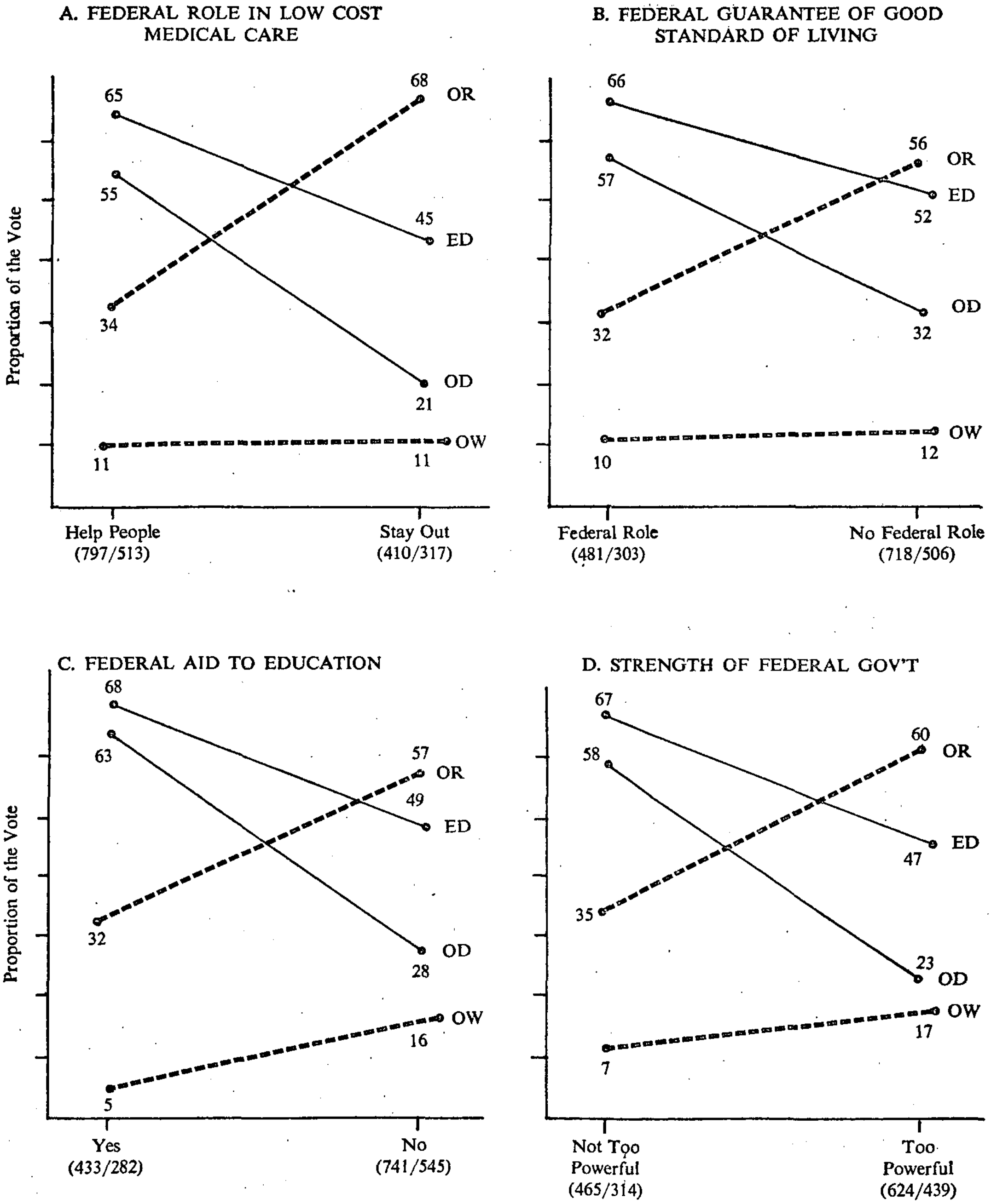
[23] McClosky et al., "Issue Conflict and Consensus Among Party Leaders and Followers;" pp. 418–19. McClosky did find some differences between the party followings on "bread and butter" issues such as farm prices, business regulation, taxes, and minimum wages. These differences are minor compared to the differences presented in Figure 8, however.

[24] See Figures 1, 5, 2, and 9 respectively for the exact wordings of the items.

[25] White, *Making of the President, 1968,* p. 371.

[26] White, p. 365.

Figure 8. The Normal Vote and Federal Role in Social Welfare*



* Because of small cell sizes the intermediate response categories, "it depends" or "both," are omitted from this figure.

Item: A. "Some say the government in Washington ought to help people get doctors and hospital care at low cost; others say the government should not get into this . . . What is your position?"

B. "In general, some people feel the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on his own . . . What is your position?"

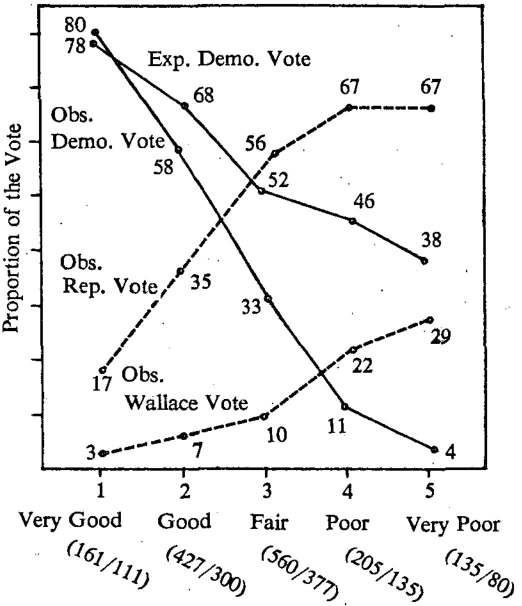
C. "Some people think the government in Washington should help towns and cities provide education for grade and high school children; others think this should be handled by the states and local communities . . . Which are you in favor of?"

D. "Some people are afraid the government in Washington is getting too powerful for the good of the country and the individual person. Others feel the government in Washington is not getting too strong for the good of the country . . . What is your feeling?"

for whom the traditional social welfare issues remained important.[27] In this respect the trend in 1968 bore a notable similarity to Truman's late surge in 1948. In their Elmira study Berelson et al. document the basis of Truman's "Fair Deal Rally." They found the swing to Truman to be sharpest among those for whom class issues were most salient.[28]

Evaluations of Johnson's Performance as President: Humphrey in Johnson's Shadow. The final issue is the public's evaluation of Johnson's performance as president. The identification of Humphrey with Johnson in the public's mind proved to be one of the most intractable of Humphrey's problems. Politically, people saw them as a single person; indeed, their judgment of Humphrey correlated with that of Johnson at a level of .70, a figure that exceeded the association of any of the candidates with their own running mates.[29] Throughout the month of September, observers wrote of the price that Humphrey seemed to be paying for the unpopularity of Johnson and the war. According to Theodore White, he was heckled at nearly every speaking engagement during that month. The Salt Lake City speech of September 30 was Humphrey's major attempt to assert his independence of Johnson's policies. While the speech may have given him some respite from dissidence, his association with Johnson appeared to remain a major burden. The consequence of Humphrey's perceived identity with Johnson is graphically illustrated in Figure 9. People who characterized Johnson's performance as "very good" gave Humphrey even more than his expected share of the vote. From that point Humphrey's vote plummeted in an almost linear fashion as the severity of judgment of Johnson mounted. From those who judged Johnson's performance as "poor" or "very poor," Humphrey received only a small fraction of the normal Democratic strength. Of all the issues considered in this analysis, this one bears the greatest relationship to defections from the normal vote.

Figure 9. The Normal Vote and Evaluation of LBJ's Performance



Item: "In general, how do you feel about how President Johnson has done his job. Would you rate his handling of America's problems over the past four years as very good, good, fair, poor, or very poor?"

Humphrey's fate may represent a case of a more general phenomenon—that the unpopularity of leaders transfers to their successors more readily than does their popularity. For example, those who thought well of Johnson's performance outnumbered those who thought poorly of it by a comfortable margin. However, Johnson's admirers gave Humphrey only a normal Democratic vote, while his detractors voted heavily against him.

Indeed, it may be a contribution to party responsibility and competition that blame's retribution survives credit's reward.[30] Parties are more likely to be sensitive to public opinion, if they believe popularity dissipates quickly, while political blunders endure in the public mind. In the process of governing, the party in power collects enemies faster than friends.

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[27] Kevin Phillips's view is the same. "Fears that a Republican administration would undermine Social Security, Medicare, collective bargaining, and aid to education played a major part in keeping socially conservative blue-collar workers and senior citizens loyal to the 1968 Democratic candidate." Kevin P. Phillips, *The Emerging Republican Majority* (Rochelle, N.Y.: Arlington House, 1969), p. 464.

[28] Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: The University of Chicago Press, 1954), Chap. 12.

[29] The correlation is between evaluations of Johnson and Humphrey on the SRC "feeling thermometer." Weisburg and Rusk, "Dimensions of Candidate Evaluation," p. 1173.

[30] We might call this the Barkley Effect, after Alben Barkley's tale of a constituent he had befriended many times who expressed to Barkley an intention to vote against Barkley in a forthcoming election. When Barkley reminded him of the many favors he had given the man in the past, the constituent responded, "Yeah, but what in hell have you done for me lately?" The story is quoted in Donald Matthews, *U.S. Senators and Their World* (New York: Vintage Books, 1960), p. 218.

In this vein the authors of *The American Voter* rely on the persistence of negative attitudes to explain the inexorable decline of majorities.[31] Thus, we have an explanation of the restoring forces that seem to create an equilibrium among competing American parties over time.[32]

Popular Control of Public Policy

Thus far, I have described the relationship of a group of issues to the 1968 vote. The strongest of these were the public's evaluation of Johnson's performance as president, reaction to the Vietnam war, and attitudes toward race and urban unrest. At this time and place, a set of conditions converged creating the unusual in politics—a cluster of issues so salient and crystalized that the popular vote for the candidate of the majority Democratic party barely exceeded 40 per cent.

In a more speculative vein, it is interesting to ponder the second major inquiry of the paper —the relationship of public attitudes to popular control of governmental policy. The problem is posed by V. O. Key's classic question: If governmental policy corrodes the vitality of a society, who is the villain—the public or its political leaders?[33] To conclude that it is the public requires evidence, Key argues, that the public severely limits the discretion of policy makers. Yet, from survey data Key notes that substantial portions of the public are unconcerned and uninformed about most concrete policies of government. Whatever limits opinion fixes, governments still enjoy wide latitude in the determination of whether to act, when to act, and what exactly to do. Thus Key concludes, leaders cannot excuse their actions by pleading the hampering restrictions of public opinion.

Key's formulation of the problem of responsibility for policy has its difficulties. One is the factual problem of distinguishing different degrees on a continuum of popular constraints on leaders' policy options. The difficulty of this judgment is lessened if the continuum is divided into three broad categories: (1) issues on which people exercise no important constraints and, thus, have no voice; (2) issues on which people have voice, but permit political leaders a wide array of policy options; and (3) issues on which voters do severely restrict the options of leaders.

Regardless of the extent of popular control over policy, a second, ethical issue remains. If we can agree that the public should not be held responsible for policy when leaders possess an array of options (categories 1 and 2), what of the case in which leaders' options are severely restricted? Many would surely argue that leaders are morally responsible for public policies no matter how much the public seems to force their hands. I will not take up this issue except to assert that severe public constraints on policy options is a *necessary* condition for holding the public responsible for a policy. Whether or not that condition is sufficient remains problematic. Caveats considered, let us turn to examples in the 1968 election of the three categories of popular control over policy.

(1) On many, perhaps most, governmental policies, the public as an electorate has *no voice*. On these policies, only the few have influence because the many have no interest and no information. Issues that do not impinge upon people's economic needs or ego defenses fall into this category, as do, no doubt, policies of abstruse complexity. Foreign policies in peace time, strategic decisions, esoteric regulatory rules—these sorts of issues are not likely to engage popular attention even though they may have a substantial impact on people's lives in the long run. Certainly political leaders must bear full responsibility for failures in policy in these areas.

(2) On other issues the public does find its voice, but it still leaves political leaders with so many options that leaders retain responsibility for the success of their choices. This interpretation of the relationship of popular preference and public policy is consistent with the views of

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[31] Campbell et al., *The American Voter*, pp. 554–55.

[32] For further empirical evidence of party equilibrium, see the list compiled by Stokes and Iverson, "On the Existence of Forces Restoring Party Competition," in Campbell et al., *Elections and the Political Order*, pp. 180–93. For other explanations of party equilibrium, see the list compiled by Stokes and Iverson, p. 181. See also the discussion of E. E. Schattschneider, *Party Government* (New York: Holt, Rinehart and Winston, 1942) pp. 93–98. Schattschneider explains the recuperative powers of the second major party in terms of its monopoly of opposition and the disadvantages for the party in power of a large majority. This latter point is one of the early statements of the size principle derived by William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), pp. 32–76.

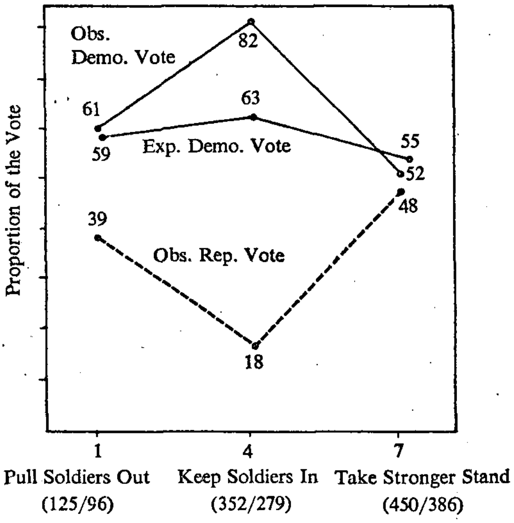
[33] V. O. Key, Jr., "Public Opinion and the Decay of Democracy." *Virginia Quarterly Review*, 37 (Autumn, 1961), 481–94. A shorter presentation of the argument appears in his book, *Public Opinion and American Democracy*, pp. 555–58. One might wonder what Key means by the failure of public policy: is failure defined by some objective standard or is it subjectively defined by people? This raises all the thorny problems of an interpersonal comparison of utilities and a social welfare function. Key seems to have some objective standard in mind, although he does not suggest who determines failure in any particular instance. I am more interested here in the issue of who influences policy making than the evaluation of the merits of policy. For the purposes of this discussion, evaluation is presumed to be the prerogative of each voter.

Key and many other voting analysts.[34] In the main, policy initiatives lie with the leaders, not voters. After a policy is enacted, voters make a retrospective evaluation of its success. The electorate's influence over such a policy is the leaders' anticipation of voters' reactions in the following election,[35] when voters will ask themselves, "How did the party in office do, and how likely is it that the opposition party would have done better?" An archetypal example is Humphrey's loss of support among voters who ranked Johnson's presidential performance as poor. It is in this sense that Key remarked, "The vocabulary of the people consists mainly of the words 'yes' and 'no'; and at times one cannot be certain which word is being uttered."[36]

Vietnam may be another example of an issue on which the electorate retrospectively said "no" to governmental policy. This interpretation is necessarily tentative because people make voting decisions across a range of issues. As a result, a majority may vote for a man in spite of his position on an issue, not because of it.[37] Thus, even when one limits the interpretation of the vote to "yes" and "no," it is difficult to infer conclusively what the judgment is on any particular issue. On the matter of Vietnam policy alternatives, one can only speculate about the nature of constraints posed by past and future elections.

On the eve of the 1964 election, only 20,000 American soldiers were in Vietnam, and President Johnson was campaigning on pledges against fighting Asian wars with American boys. What could the 1964 vote have told him about popular support for various war options? In brief, it could have told him anything he cared to believe.

Figure 10. The Normal Vote and Escalation/De-escalation in 1964



Item: "Which of the following do you think we should do now in Vietnam?"

1. Pull out of Vietnam entirely.
2. Keep our soldiers in Vietnam but try to end the fighting.
7. Take a stronger stand even if it means invading North Vietnam.

Figure 10 presents a normal vote analysis of the escalation/de-escalation issue in 1964. The data could be interpreted as support for "pull[ing] out of Vietnam entirely." Johnson received 62 per cent of the votes from the proponents of this option—a substantial majority, and three percentage points better than a normal vote would have predicted. On the other hand, the data also show strong support for "keep[ing] our soldiers in Vietnam but try[ing] to end the fighting." From this group he received a startling 82 per cent of the vote, 18 percentage points more than a normal vote would have predicted. Finally, the data also reveal popular support for the option of "tak[ing] a stronger stand even if it means invading North Vietnam." Although he failed to win the expected Democratic vote within this group, he still claimed a majority. And, it should be noted, the advocates of escalation easily numbered the largest of the war opinion groups. Looking at the 1964 data and noting Pomper's evidence that 1964 Johnson voters were still supporting Johnson's policies in 1966, one must conclude, as Pomper did, that Johnson did not violate an electoral mandate in spite of the fact that he ignored his own campaign pledges.[38] The 1964 election foreclosed none of Johnson's war options. He and other political leaders formulated the escalation policy and rightly should have borne full electoral responsibility for it.

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[34] This presentation owes much to a lucid discussion by Gerald M. Pomper, *Elections in America: Control and Influence in Democratic Politics* (New York: Dodd, Mead, 1968), Chap. 10. To use Tingsten's phrase (if not all of the rationale that produced it), "The public . . . [is a] consumer rather than [a] producer of political demands and candidates." Herbert Tingsten, *The Problem of Democracy* (Totowa, N.J.: The Bedminster Press, 1965), p. 198. See also, Key, *Public Opinion and American Democracy*, pp. 472–78; Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown 1964), Chap. 7; Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), Chap. 3; and R. J. Benewick, A. H. Birch, J. G. Blumler, and Alison Ewbank, "The Floating Voter and the Liberal View of Representation," *Political Studies*, 17 (June, 1969), 177–195.

[35] Friedrich's Rule of Anticipated Reactions. Carl J. Freidrich, *Man and His Government* (New York: McGraw-Hill, 1963), pp. 199–215.

[36] V. O. Key, Jr., *Politics, Parties, and Pressure Groups*, 5th ed. (New York: Crowell, 1964), p. 544. The quote also appears in Pomper, *Elections in America*, p. 252.

[37] A party could win an electoral majority by systematically taking positions favored by a minority. Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: The University of Chicago Press, 1956), pp. 124–31. This situation Downs calls a "coalition of minorities." It stands as a contrast to a "passionate majority," which occurs only when there is a consensus on which issues are important as well as a consensus of preferences on the issues. Downs, *Economic Theory of Democracy*, pp. 64–69.

By 1968, escalation had changed the context of the options confronting the electorate. Rather than 20,000 men in Vietnam, there were 530,000. Sixteen thousand men had died, and America's prestige was at stake. As we have seen, the public's reaction to a continuation of the existing war policy was negative. Humphrey won a majority of the votes of people endorsing a centrist, status quo alternative: "keep our soldiers in Vietnam but try to end the fighting." He fared less well among those who would pull out entirely; he fared disastrously among those who would escalate the war still further, a group of voters who gave both Wallace and Nixon the greatest proportions they received from any Vietnam attitude category. (See Figure 2.) The common thread of the views of those who deserted Humphrey —people who would withdraw rapidly or who would sharply escalate the fighting—would seem to be a desire to end the engagement quickly. As in the Korean war, an official Vietnam policy of a war of limited aims and lengthy duration confronted a public unwilling to support war on those terms.[39]

Though one cannot marshal evidence about events that never happened, the Johnson Administration appeared to lose more electoral support by its pursuit of an extended limited war than it would have lost either by a policy of withdrawal (even after the buildup of men) or by a precipitous escalation of the conflict. This view of the electoral dangers of limited conflict is consistent with the views of Key, Kahn, and Brzezinski and Huntington.[40] The public does not necessarily *prescribe* a particular policy, but it can *proscribe* a policy—e.g., the option of extended limited war. Even after the 1968 election, Rosenberg, Verba, and Converse argued that policy makers retained a wide array of options for ending the war.[41]

> . . . the President is not particularly pressured by public opinion to engage in one kind of policy rather than another—to escalate or to de-escalate the war. Rather, the public would support a variety of initiatives. What the public wants is an end to the war. Thus it will judge the policies of the President retrospectively in terms of how successful they are in this direction.

If political leaders possessed a wide array of options in Vietnam, then are there issues on which the electorate does severely limit the options of political leaders? If there are, what must the structure of political attitudes on such an issue be? For this close relationship of opinion to policy to exist, the following conditions are surely necessary: (1) A substantial number of people must have intense views about specific policy alternatives. (2) People must have information about the views of competing candidates on the issue. (3) These attitudes must be fairly impervious to short-run manipulation by political leaders. (4) The balance of opinion on the issue must be such that the winning candidate's choice of an issue position significantly affects his probability of being elected. Clearly, these are necessary, not sufficient, conditions for influencing policy through the electoral process. There are other channels for influencing policy in our system, all with their own conditions.

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[38] Pomper, *Elections in America,* p. 252.

[39] See the discussion of the electoral impact of the Korean War by Warren E. Miller, "Voting and Foreign Policy," in *Domestic Sources of Foreign Policy,* ed. James Rosenau, (New York: The Free Press, 1967), pp. 213–30.

[40] Key, *Public Opinion and American Democracy,* p. 261; Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1960), p. 418; Zbigniew Brzezinski and Samuel P. Huntington, *Political Power: USA/USSR* (New York: Viking Press, 1964), p. 414. These and other writers are discussed in a good article on this issue, Kenneth N. Waltz, "Electoral Punishment and Foreign Policy Crises," in Rosenau, pp. 263–294. Waltz does not agree with the general conclusion that the public will not support a limited war. Rather, he blames the lack of broad-based support for the Korean War on the failure of the Truman administration to explain its policy adequately to the public because of his administration's own uncertainty over its goals (pp. 279–80). Waltz does not, however, either have any direct evidence about the impact of opinion on votes or any indirect evidence about the intensity of opinion on these issues. Lacking this type of information, one cannot easily judge how governmental policy will affect votes or how votes will affect policy. This same problem confronts those who have found permissive majorities supporting the Vietnam war. See, for example, Sidney Verba, Richard A. Brody, Edwin B. Parker, Norman H. Nie, Nelson W. Polsby, Paul Ekman, and Gordon S. Black, "Public Opinion and the War in Vietnam," *American Political Science Review,* 61 (June, 1967), 321, and William R. Caspary, "The 'Mood Theory': A Study of Public Opinion and Foreign Policy," *American Political Science Review,* 64 (June, 1970), 546. For a sobering discussion of the difficulties of interpreting the significance of opinion polls and support for the Vietnam War, see Philip E. Converse and Howard Schuman, "'Silent Majorities' and the Vietnam War," *Scientific American,* 222 (June, 1970), 17–25.

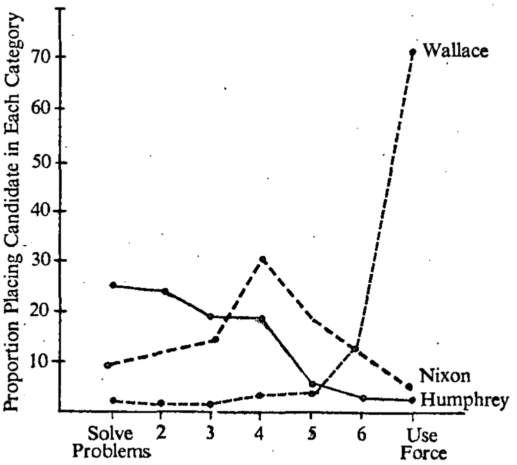
[41] Rosenburg, Converse, and Verba, *Vietnam and the Silent Majority,* p. 38.

What contemporary issues might satisfy these four conditions? The issues of race and public order seem the obvious candidates. Even in the quiescent period of the late 1950s, there was evidence of a close relationship between opinion and policy on racial attitudes. Miller and Stokes, for instance, found a correlation of .65 between congressional roll-call votes and constituency opinion on civil rights issues.[42] Furthermore, the link between the two was the representatives' inclination to vote their *perceptions* of their constituencies' opinion; they were not simply voting their own attitudes, which happened to coincide with the majority opinion in their districts. Let us see how the race issue in the elections of 1964 and 1968 might satisfy the four conditions:

(1) *A substantial number of people must have intense views about specific policy alternatives.* Survey questions are rarely framed in a manner that presents reliable evidence on either the direction or the intensity of views on specific policies. One exception in the 1964 SRC election study is a set of items probing knowledge of and support for the 1964 Public Accommodations Law. All but 23 per cent said they knew of the new law (and as shown below, their knowledge of the candidates' stands indicates people were not simply giving the socially desirable answer). Of those having an opinion on the law, 90 per cent said their minds were made up. No such questions were asked in the SRC election studies on the 1965 Voting Rights Law or the 1968 Open Housing Law. Nevertheless, we can be reasonably confident that the controversies over their passage served to crystalize intense views on these policies. Negroes voted more than 90 per cent Democratic in 1964 and 1968, a display of cohesion that reflects the interests they saw at stake in the two elections.[43]

(2) *People must have information about the views of competing candidates on the issue.* In 1964 the level of information on the public accommodations law was remarkably high. Of the 77 per cent who said they had heard of the law, 95 per cent knew Johnson favored it and 84 per cent knew Goldwater opposed it. Even in 1968, when differences between the Republican and the Democratic candidates were less striking, voters had little trouble sorting out the candidates' stands on racial issues. Figure 11 presents a distribution of people's perceptions of the candidates' positions on the urban unrest issue. The modal perceptions of Humphrey, Nixon, and Wallace were distinctly left, center, and right, respectively—perceptions that were quite accurate in view of the different positions of the parties and the candidates on issues of race and urban violence.[44] If one limits the sample to those who viewed the race issue as important in determining their vote, the distinctness of the perceived differences among the candidates is even more pronounced.

Figure 11. Distribution of Perceptions of Candidates' Stands on Urban Unrest



Item: See Figure 5.

(3) *Attitudes on the issue must be fairly impervious to short-run manipulation by elites.* Almost every survey of the literature on mass persuasion emphasizes the difficulty of chan-

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[42] Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," in Campbell et al., *Elections and the Political Order,* p. 359.

[43] Actually, the 1964 SRC sample does not include a single Negro Goldwater voter. Fred I. Greenstein, *The American Party System and the American People,* 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 26. In France, religion has been an issue having a similar degree of crystallization and polarization. See Philip E. Converse and Georges Dupeux, "Politicization of the Electorate in France and the United States," in Campbell et al., *Elections and the Political Order,* pp. 289–90.

[44] Sources of platform and campaign positions are White, *Making of the President 1968,* Lewis Chester, Godfrey Hodgson, and Bruce Page, *An American Melodrama* (New York: Viking Press, 1969); *Congressional Quarterly Almanac,* pp. 942–1046; and *The Presidential Nominating Convention: 1968* (Washington: Congressional Quarterly, 1968).

ging popular attitudes when attitudes are well formed and when the time available to the elites is reasonably short (as in a campaign).[45] Certainly, racial attitudes should be among the most difficult for opinion leaders to influence, for they are learned early in life and are often central to people's ego defenses.[46] Thus, we can be reasonably sure that if we discover a close relationship between popular attitudes and the enaction of public policies on race issues, the link cannot be explained away by arguing that political leaders persuaded voters to support policies the voters previously resisted.

(4) *The balance of opinion on the issue must be such that the winning candidate's choice of an issue position significantly affects his probability of being elected.* It is easier to state this condition than to present evidence appropriate to its test. In the first place, the importance of a particular issue in an election differs for each candidate (for example, whether, in Downs' terms, a candidate's victory is based on a "coalition of minorities" or on a "passionate majority").[47] One illustration of this problem is the difference between the importance of the race issue for Nixon and Humphrey in 1968. Nixon won the election even though he lost more than 90 per cent of the Negro vote. Presumably, Humphrey needed all of those votes to have had any prospect of victory. In the second place, we have no analytical technique for determining the effect of alternative issue positions on each candidate's electoral coalition.[48] Our only approach to the analysis of the fourth condition is an indirect one—examining the statistical relationship of beliefs on issues to votes.

In 1964 the relationships between civil rights issues and votes were substantial. The gamma association of the vote with support for the public accommodations law was .57; with the establishment of a fair employment practices commission for Negroes, .47; with views on federal enforcement for school integration, .37. These links between civil rights positions and votes continued in 1968, as I argued in the first section of this paper.

The position that racial attitudes are closely related to public policy involves an apparent contradiction. If Humphrey's losses in 1968 were statistically related to racial attitudes, why did Congress in that same year pass an open housing law covering 80 per cent of all housing units? One answer is that there is no survey evidence of an increase in the number of white people opposed to traditional civil rights goals such as public accommodations, voting rights, and open housing.[49] There is greater fear of Negro crime and urban violence. This attitude is mirrored in the fact that the open housing law contains an anti-riot section.[50] Most people apparently believe that Negroes now enjoy legal equality and that the remaining social and economic inequalities are the responsibility of Negroes themselves.[51]

The evidence on racial attitudes and civil rights laws is undoubtedly insufficient to support a contention that Congress scarcely had time to leap to the lead of a mob bent on liberal changes in civil rights laws. On the other hand, it is so commonly assumed that changes in public policy have *preceded* changes in public opinion on civil rights issues that a second look is warranted.[52] The sum of the evidence indicates that racial issues were voting issues in 1964 and 1968, and that majorities of the voters supported the major civil rights laws passed in those years (though we note the warning of Converse and Schuman that the wording of an item has a major effect on which response category attracts majority support).[53] In my view the following argument merits attention: Congress ran few risks in its support for civil rights laws in the 1960s—not because an apathetic public paid little attention to policy proposals, but because on these issues Congress carefully stayed within the relatively narrow constraints posed by a watchful electorate.

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[45] Joseph T. Klapper, *The Effects of Mass Communications* (Glencoe: Free Press, 1960).

[46] The literature on this subject is mammoth. See Frank R. Westie, "Race and Ethnic Relations," in *Handbook of Modern Sociology*, ed. Robert E. L. Faris, (Chicago: Rand McNally, 1964), pp. 576–618.

[47] Downs, *Economic Theory of Democracy*, pp. 64–69.

[48] A pioneering attempt to measure the impact of issue positions on votes is found in Ithiel de Sola Pool, Robert P. Abelson, and Samuel Popkin, *Candidates, Issues, and Strategies: A Computer Simulation of the 1960 and 1964 Presidential Elections* (Cambridge: The M.I.T. Press, 1965). No one has completed a rigorous development of the technique, however.

[49] Converse *et al.*, "Continuity and Change in American Politics," p. 1086; Herbert H. Hyman and Paul B. Sheatsley, "Attitudes Toward Desegregation," *Scientific American*, 211 (July, 1964), 16–23; Paul B. Sheatsley, "White Attitudes Towards the Negro," *Daedalus*, 95 (Winter, 1966), 217–38. Perhaps a proper interpretation of the backlash is that racial opinions have become increasingly salient for the declining numbers of people holding prejudiced attitudes.

[50] *Congressional Quarterly Almanac*, pp. 152–68. This report contains a good description of the act and an account of the politics of its enactment.

[51] *Gallup Opinion Index* (July, 1968), 19–20.

[52] See, for example, Sheatsley, "White Attitudes Towards the Negro," p. 221. This position is rarely stated explicitly in the literature, but it is implicit in much of the evidence that political, business, and social leaders are more liberal than the public on civil rights issues.

[53] Converse and Schuman, " 'Silent Majorities' and the Vietnam War," pp. 17–25.

The time to test the argument on existing civil rights laws is gone, for we cannot reconstruct a public past. Yet, determining the linkage between public opinion and policy is so critical a problem that it behooves us to ask policy questions of the public. We need to ask more than general questions about political attitudes. We need to know what people think about particular policies—the levels of information, intensity, and support for specific options. Without succumbing to hopeless naiveté about the interest and information of the electorate, the opinion-policy linkage on the issues of race and public order is evidence for the rich payoff of such an approach.

Summary and Conclusions

Converse developed the concept of the normal vote in order to interpret the force of the religious issue in 1960. His challenge was to analyze voting changes between 1956 and 1960 without using 1956, a deviating election, as a baseline for the measurement of change. The normal vote, an empirical estimation of an abstract concept, became the baseline, and the impact of the religious issue was measured as a deviation from the normal vote.

The logic of the normal vote presumes that the force of issues constantly changes. This logic has been too often ignored by those who assume the immutability of political behavior. The electorate of 1956 was presented in *The American Voter* as being little moved by ideology or public issues. But 1956 was a deviating election in a quiescent political era, and it should be of little surprise to find a minimal amount of issue voting in that apolitical period. The importance of issues in 1968 stands in striking contrast to 1956. Beliefs about Vietnam, race and urban unrest, and Johnson's performance as president were all highly related to the vote in 1968. Time has told us of the unusual nature of the 1956 election. Undoubtedly, the future will reveal the atypicality of 1968, as American parties seemed to be in a stage of realignment or disintegration. Surely we should expect issues to be more important in these times than in periods of stability in party strength. After what appears to be a transition period, issue voting may once again decline to the level of the 1950s.

A second, more speculative argument of the paper is that issues similar in the magnitude of their relationship to the vote can pose very different restrictions on the discretion of leaders in policy making. On some issues the electorate exercises no effective constraints on leaders' policy choices. On others, the electorate permits political leaders a wide array of options at the time of the adoption of policy, while passing a retrospective judgment on such choices in subsequent elections. It is argued that the issues of Vietnam and Johnson's performance as president are examples of this type of public control of policy. Finally, there may be issues on which the public rather severely limits the options of leaders at the time of the adoption of policy. It is difficult, surely, to amass the evidence necessary to determine which issues could properly be placed into this final category. If any issues of 1964 and 1968 meet the tests, race and public order do. It may be, then, that the electorate must assume final responsibility for the success with which the United States solves its racial problem.

Appendix I. Inter-Association Matrix of 15 Issues

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1		17	16	16	25	17	13	12	17	13	00	04	07	01	03
2	30		35	32	26	14	06	03	04	13	13	12	07	08	13
3	24	59		28	23	25	15	11	17	22	14	14	16	15	23
4	27	58	47		32	25	22	14	23	22	13	12	18	11	15
5	42	46	39	56		19	15	12	16	14	09	09	13	07	11
6	31	29	43	45	36		43	26	35	27	14	14	29	16	26
7	28	13	30	43	31	72		26	35	19	13	15	34	17	25
8	23	06	21	29	24	58	65		47	14	03	06	22	10	14
9	24	07	29	41	28	60	67	77		24	06	11	26	15	17
10	17	21	29	34	21	42	36	25	31		16	24	18	14	20
11	00	23	23	22	15	24	25	06	09	20		43	11	06	15
12	05	19	19	19	14	22	29	11	15	29	53		16	12	14
13	10	13	27	31	23	48	59	45	42	24	17	21		28	36
14	01	16	25	20	13	28	33	22	26	21	10	17	45		29
15	08	29	42	30	25	49	50	37	34	39	29	27	61	52	

The upper diagonal contains the Kendall Tau; the lower diagonal contains the Goodman-Kruskal Gamma. All associations are positive if the left position is defined as favoring Johnson, a strong federal role in social welfare, civil rights, de-escalation of the Vietnam war, and civil liberties. The items are the following:

1. LBJ's performance. Fig. 9.
2. Medical care. Fig. 8.
3. Standard of living. Fig. 8.
4. Aid to education. Fig. 8.
5. Power of government. Fig. 8.
6. Speed of civil rights. Fig. 1.
7. Negro violence. Fig. 6.
8. Residential segregation. Fig. 6.
9. Segregation in general. Fig. 6.
10. Urban unrest. Fig. 5.
11. Escalation/de-escalation. Fig. 2.
12. Immediate withdrawal/military victory. Fig. 3.
13. Legal marches. Fig. 7.
14. Civil disobedience. Fig. 7.
15. Mass demonstrations. Fig. 7.

Appendix II. The Comparative Importance of 15 Issues

Issue	Short-Term		Partisan or Long-Term	
	Magnitude	Rank	Magnitude	Rank
1. LBJ's performance	8.91	1	10.56	1
2. Medical care	6.40	7	9.60	2
3. Standard of living	4.73	13	6.39	6
4. Aid to education	7.03	5	8.45	4
5. Power of government	7.79	3	9.32	3
6. Speed of civil rights	6.16	8	4.13	8
7. Negro violence	5.78	10	2.90	9
8. Residential segregation	5.19	11	1.25	14
9. Segregation in general	7.01	6	4.29	7
10. Urban unrest	8.78	2	6.59	5
11. Escalation/de-escalation	7.21	4	1.18	15
12. Immediate withdrawal/military victory	6.12	9	2.70	10
13. Legal marches	4.80	12	1.65	13
14. Civil disobedience	1.06	15	2.53	11
15. Mass demonstrations	3.18	14	2.07	12

If we are to compare the importance of a series of issues, we must have a quantitative measure. The measures above, which are based on an analogue of analysis of variance, offer the advantages of having a straightforward interpretation and of reflecting the existence of different numbers of cases across issue categories.

(1) Partisan or long-term component. The long-term effect of an issue is manifest in a change in the expected Democratic proportion of the vote across issue categories. A nonlinear measure of this change is the average of the absolute values of the deviations of the expected Democratic vote in the response categories from the expected Democratic vote for the whole item sample. The importance of each category in the total average is weighted by the proportion of the total cases in that category. This simple, percentage-point average is computed in this fashion.

If:

N = Number response categories for an issue,

$E_j \cdots n$ = Expected Democratic vote in each issue category j, and

$WG_j \cdots n$ = Ratio of the number of party identifiers and independents in category j over the average number of party identifiers and independents in each category (the weighting factor),

Then:

$$GE = \sum_{n}^{j=1} E_j WG_j / N$$ (the grand expected Democratic vote);

$$L = \sum_{n}^{j=1} \text{Abs. Val. } (E_j - GE)(WG_j)/N$$ (the partisan or long-term component).

To use Figure 1, the long-term component of speed of movement on civil rights, as an example, the grand expected Democratic vote is 57.4 per cent. The average deviation of the expected Democratic vote in each category from 57.4 (weighted by the proportion of cases in each category) is *4.1* percentage points. If there were no change from category to category, the category expectation would be the same as the grand expectation, that is 0.0.

(2) Short-term component. The short-term component of an issue is manifest in changes in the deviation of the expected from the observed Democratic vote across issue categories. That is, for all the voters included in Figure 1, Humphrey actually received 41.0 per cent of the vote, 16.4 percentage points below his expected vote in a normal election. If Humphrey had lost this same 16.4 percentage points in every category, then other factors than this one would have to account for his deficit. Thus, the average variation of the actual losses in each category (0.5, 9.8, and 21.2 points) from the grand or overall deficit (16.4) is a measure of the short-term component of this issue. As before, the average is weighted by the number of cases in each issue category.

If:

$WA_{j\ldots n}$ = Ratio of the number of voters in category j to the average number of voters in each category (the weighting factor), and

$O_{j\ldots n}$ = Observed Democratic vote in each category j,

Then:

$$D_j = E_j - O_j;$$

$$GD = \sum_{n}^{j=1} (D_j)(WA_j)/N$$ (grand deviation of the observed from the expected Democratic vote);

$$S = \sum_{n}^{j=1} \text{Abs. Val. } (D_j - GD)(WA_j)/N$$ (the short-term component).

Comment: The Assessment of Policy Voting*

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The voter has the option of voting or of abstaining; if he votes, he chooses among candidates. By "policy voting" we mean the extent to which these behaviors are caused by the voter acting in accord with his policy preferences. The question with which we are here concerned is: How do we know whether and to what extent "policy voting" has taken place in a given election? The process of obtaining this knowledge is what we mean by the "assessment of policy voting."

We will argue that the assessment of policy voting depends upon the specification of necessary and sufficient conditions for the influence of policy attitudes in the voting decision. But prior to this specification and in order to justify it, we will review existing assessment techniques.

Policy Voting as a Resultant of Short-Term Forces

The causal significance of policy preferences for the vote is generally treated as inhering in a given election. The issues that affect one election do not necessarily affect other elections past or future. Policy preferences, the voter's attitude toward the issue, are by these criteria treated as "short-term" forces.

The categories of short-term forces are conceived of by most scholars as persisting from election to election. However, the specific content of the categories and the impact of a given short-term force have been found to be highly variable.[1] Because such evidence as we have from the voting studies indicates that policy preferences are extraordinarily labile,[2] our theories of voting do not employ such concepts as "stable" foreign affairs or labor relations attitudes in the way that they employ the concept of "stable partisan attitudes."

There is a mutual reinforcement between the conception of short-term effects of issues and the cross-section or short panel sample survey design. The learning of policy satisfaction/dissatisfaction over time which hypothetically could affect the strength and eventually the direction of partisanship, cannot easily be studied with such a data-gathering procedure. It is unusual in the cross-section to see partisan identification treated as a response partly learned from the reinforcement of policy preferences. Gerald Pomper, for example, does not consider the possibility that partisan realignment would produce exactly the same correlations that he attributes to increasing "ideological clarity."[3] We are certainly not asserting that if the panel data were available, they would show a massive effect on partisan identification of the feedback of policy satisfaction. Clearly, we do not have such data available.

In our leading models of voting, partisan identification is said to be learned early as a part of primary socialization into family and class. The issue content of partisan identification, to the extent there is any, reflects these familial and class sources.[4] This content in turn affects the attraction to issues (their salience for the voter) as well as the voter's stands on new issues as they arise. David RePass's notion of "issue partisanship" exemplifies this conception.[5]

Our leading models, thus, conceive of partisan identification as a "standing decision" which may reflect or may have once reflected

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[1] Donald Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science* 60 (March, 1966), 19–28.

[2] See, for example, Philip Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent,* ed. David Apter, (New York: The Free Press, 1964), pp. 206–261 and David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martins, 1969).

[3] Gerald Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review,* 66 (June, 1972), 415–428.

[4] V. O. Key, *Politics Parties and Pressure Groups,* 4th ed. (New York: Crowell, 1958), p. 238.

[5] David RePass, "Issue Salience and Party Choice," *American Political Science Review,* 65 (June, 1971), 389–400.

Table 1. Vote for Humphrey by Party Foreign Policy Performance by Partisan Identification

Party Identification		Party Preferred on Foreign Policy[a]		
		Democrats	Same by Both	Republicans
Strong Democrats	%	92[b]	85	50
	(N)	(72)	(102)	(16)
Medium Democrats	%	87	56	29
	(N)	(29)	(151)	(35)
Independents[c]	%	72	28	4
	(N)	(24)	(144)	(78)
Medium Republicans	%	50	9	7
	(N)	(6)	(74)	(68)
Strong Republicans	%	—	4	2
	(N)	(0)	(27)	(85)

[a] "Looking ahead, do you think the problem of keeping us out of a bigger war would be handled better in the next four years by the Democrats, by the Republicans, or about the same by both?"

[b] Entries are per cent of the two-party vote going to the Democratic candidate, Humphrey; data from the SRC/ICPR 1968 American National Election Study.

[c] Those in this row responded "Independent" on the first probe.

preferences for a direction of policy (perhaps still viable and rational, perhaps not) as well as a persistance of habitual behavior not related to public policy.[6] Even occupational mobility, which may bring the voter into a new relationship with the consequences of public policy, does not much affect the voter's partisanship.[7]

Thus as political attitudes go, partisan identification, as currently conceived, is an attitude that does not yield to experience.[8]

Assessing Short-Term Effects

The notion of stable partisanship gives rise to techniques for assessing the extent of policy voting: V. O. Key, for example, uses votes contrary to partisanship among those who express dissatisfaction with their party's past performance or in their expectation of its performance in the future to estimate the impact of issues on the vote.[9]

Table 1 is constructed in the manner of those Key offers as evidence that issues are considerations in the collective electoral decision.[10] Since in this table the proportions voting for Humphrey decrease monotonically along both columns and rows, the table shows both partisanship and performance evaluation related to patterns of voting for or against the candidate of the Democratic Party. But Table 1 does not unambiguously offer evidence of the impact of the lack of confidence in President Johnson's performance in the field of foreign affairs.

The ambiguity in such tables arises from three sources: (1) From the lack of a null model against which to compare observations; (2) from uncertainty about the direction of causality between the issue assessment by the voter and his vote; and (3) from the fact that the *same* defecting votes may be explained by other variables.

We need a null model in order to assess the strength of the effect of an issue. Suppose there were no other factors in the voting decision except partisanship and a given issue. Under the null model, that is, under the circumstances in which the issue had no short-term effect, how would this table appear? Key's interpretations seem to imply that, for party identifiers, the six Democratic identifier cells should show a one hundred per cent Democratic vote and the six cells for Republican identifiers should show a zero per cent Democratic vote. It is not clear what the expected Democratic vote among In-

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[6] V. O. Key and Frank Munger, "Social Determinism and Electoral Decision: The Case of Indiana," in *Public Opinion and Politics*, ed. William J. Crotty (New York: Holt, Rinehart and Winston, 1970), p. 253.

[7] James A. Barber, Jr., *Social Mobility and Voting Behavior* (Chicago: Rand McNally, 1970), p. 114.

[8] Not all conceptions of partisanship reflect this point of view: See, for example, Arthur Goldberg's "Model V", "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review*, 60 (December, 1966), 920–921. In this model Goldberg shows partisanship to be affected by partisan attitudes which include issue orientations. James L. Sundquist (*Politics and Policy* [Washington: Brookings, 1968]) finds the "[p]arty allegiances of some voters changing" in response to presidential performance (p. 438).

[9] *Public Opinion and American Democracy* (New York: Knopf, 1961), pp. 472 ff.

[10] See, for example, V. O. Key's Tables 18.4 and 18.5. *Public Opinion* . . ., p. 473 and p. 475.

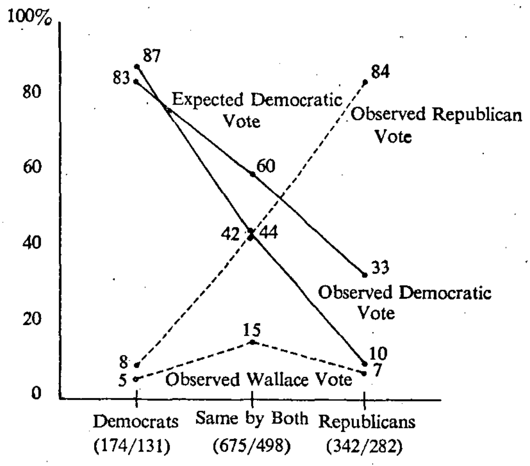

Figure 1. Attitudes toward war handling capabilities of the parties and presidential vote.

dependents would be. This null model cannot be the one Key intended. It denies any relevance to the strength of partisanship and Key uses this information. But how do we modify the null prediction to take account of the strength of partisanship? How much should we discount the expected vote of less than "strong" partisan identifiers?

A handy answer to our need for a null model is found with Converse's concept of the "normal vote."[11] Converse argues that the distribution of partisan loyalties among those with a given attitude or among those who subscribe to a given issue position, when weighted by the voting propensities associated with strength of partisanship, yields an expected percentage voting Democratic for the like-minded. Since this percentage reflects only the distribution of stable partisan loyalties, it is exactly the estimate of the expected vote in the absence of short-term forces that we need for the null model. The extent of the short-term effect of the issue is then estimated by the comparison of observed voting percentages with the percentages derived from the null model.

Applying this technique to the distribution of responses to the question of "which party can better handle the problem of keeping us out of a bigger war," we can see more clearly the processes underlying Table 1. Figure 1 displays this analysis. The distribution of partisan loyalties across positions on this issue leads us to expect a very substantial long-term effect. Compared to the fifteen issues examined in Richard Boyd's article, this policy issue with a "magnitude of long-term effect" equal to 12.9 would rank first.[12] A short-term component is also present. Those voters who did not feel positive about the past or prospective performance of the Democrats in the field of foreign affairs defect at a rate higher than that predicted by the null model. Compared with the issues Boyd examines, the short-term component is modest. With a magnitude of $S = 4.77$, this issue ranks between the twelfth and thirteenth issues.[13]

"Normal vote" analyses help us to understand the effect of a given issue in the electorate at large. But, they share with V. O. Key's technique ambiguity about the direction of causality between attitudes and behavior and the problem of the possible continued re-explanation of the same voting acts. Both techniques assume that the issue attitudes exist prior to and thus can be a cause of the vote. Clearly, however, "rationalizing" of one's vote or intention to vote contrary to one's partisanship, through finding fault with the party's performance, would produce tables that look exactly like those produced by the process in which negative views of performance caused defecting votes. In fact, if causality runs from opinion to behavior for some citizens (the "policy voters") and from behavior to opinion for other citizens (the "rationalizers") tables such as this will still look the same. If, however, voters are simply rationalizing a defection which is otherwise caused, and if we cannot distinguish them from those who switch their vote on the basis of policy considerations, we are in danger of overestimating the extent of policy voting by the number of such voters.

Exploring Rationalization

We know of no ironclad way, within a cross-section sample survey on a given issue *considered by itself*, of disentangling these substantively different processes. But if we are willing to think of rationalizing as a general response set, its effects can be examined.

Think of the repeated naming of one political party as "best able" to solve important problems or as "closer" to one's own policy position as a measure of response consistency. Suppose we are willing to assume that those who rationalize their vote on one issue, by letting their vote dictate their stand, are apt to attempt to achieve psychological balance in the same way on a set of issues; then it is clear that the rationalizers will appear among those with high consistency scores.

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[11] Philip Converse, "The Concept of a Normal Vote," in Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *Elections and the Political Order* (New York: Wiley, 1966) pp. 9–39.

[12] Richard Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), Appendix II.

[13] It might be thought that there is a necessarily negative relationship between the magnitude of short and long-term effects. On the contrary, across the fifteen issues examined by Richard Boyd, the sizes of these two components are highly *positively* correlated ($r_s = .58$; $p < .025$). This suggests that, with some notable exceptions, previously politicized issues are the ones that get activated for and used by the voter during the campaign.

If rationalization is *one way* of achieving a high party/issue consistency score, we can use this knowledge to test the *implausibility* of the hypothesis that rationalization causes the findings to be attributed to policy voting. Given an index of consistency, if the rationalization hypothesis is not justified, we will not find especially pronounced "anti-Democratic" issue consistency scores among those Democrats who vote against their party's candidate. We can only assess the *absence* of rationalization as a plausible explanation for the findings in the tables of the Key and Boyd type because other processes besides rationalization can lead to high consistency scores. If Democrats who defect are not pronouncedly more anti-Democratic than those who do not, rationalization is excluded; if they are, the question of causality is left unsettled.

Based on responses to two clusters of items in the 1968 SRC/ICPR pre- and post-election surveys, we have developed indices of partisan response consistency: The "Partisan Issue Alignment Index" reflects the degree to which the respondent felt the Democratic Party is the one to handle the "most important problems faced by the government in Washington." The index mirrors David RePass's approach of permitting the voter to identify the issues he feels are important.[14] The index summarizes, on a 19-point scale—*viz.*, +9 (pro-Democratic) to −9 (anti-Democratic) the pattern of responses to the "first," "second," and "third," most important problems identified by the respondent.[15]

Our second index, "Expectation of Policy Satisfaction," summarizes the pattern of response to 11 forced-choice policy items. It describes the degree on a 23-point scale—*viz.*, +11 (pro-Democratic) to −11 (anti-Democratic) to which a voter feels the positions he prefers will be carried out by the Democratic Party.[16]

Comparing the means and distributions of the two indices in the subsamples voting for and against Humphrey (Tables 2a and 2b), we find: (1) Those who vote against Humphrey are on the average less "pro-Democratic" in their assessment of the policy performance of the Democratic Party than are those who vote for him; this is true in every group of party identifiers. (2) The distributions about these means are such as to suggest that, within party identification groups, voters for and against Humprhey by virtue of their policy attitudes constitute separate and distinct subgroups. (3) These findings are true for salient issues and for those considered without regard to their salience for the voter.

These findings, of course, replicate David RePass's finding of the strong relationship between "issue partisanship (or alignment)" and voting choice in the 1964 Election. They do not, however, eliminate the ambiguity about whether attitudes are, as RePass contends, "a powerful influence . . . on voting choice"[17] or whether voting choice is a powerful influence on attitudes.

When we control the comparison of Humphrey and Nixon voters' Partisan Issue Alignment by their opinion on "which party would better handle the problem of keeping us out of a bigger war," (Table 3) we find evidence which raises questions about the rationalization interpretation.

Strong Democrats who think the Republicans can handle the war problem better and

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[14] David RePass, "Issue Salience. . . ."

[15] In the case of each of the three most important problems, the respondent was asked which party, if either, would carry out the policy he favored. The Partisan Issue Alignment Index sums the responses to these probes with weights 5, 3, and 1 attached to the first, second, and third most important problems, respectively. Thus, if a voter named the Democrats on all three probes, his weighted score would be +9; if he named the Republicans or Wallace, his score would be −9. On each probe, the "Democrats" response was scored +1, the "Both/Either" response was scored 0, and the "Republicans" or "Wallace" response was scored −1. It is these scores which are weighted and summed to form the index.

[16] The Expectation of Policy Satisfaction Index expresses the degree to which the respondent, on eleven policy questions, saw the Democrats carrying out policies which he favored or associated the Republicans with policies of which he disapproved. The eleven policies covered: Aid to education, the growth of governmental power, federal medical programs, aid to employment, fair employment practices, integration, public accommodations, foreign aid, negotiations with Communist nations, trade with Communist nations, and Vietnam policy. On each issue, if the respondent favored a point of view and associated the Democrats with that point of view or if he expressed opposition to a policy and associated the Republicans with that policy position, he received a score of +1; if the opposite conditions obtained, he received a score of −1; if he did not respond to the policy or if he did not identify a party with one stand or the other, his score on that item was 0. The index sums these scores over the eleven issue areas.

[17] David RePass, "Issue Salience . . .," pp. 399–400.

Table 2a. Response Consistency, Party Identification and the Vote: Partisan Issue Alignment Index

Party Identification	Voters for Humphrey			Voters Against Humphrey[a]			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
Strong Democrats	4.70	4.3	188	−0.68	5.2	34	6.52	.005
Medium Democrats	1.82	4.4	139	−2.89	4.2	102	8.31	.005
Weak Democrats	3.14	4.6	49	−3.14	3.7	44	7.14	.005
Independents	1.35	2.7	20	−3.59	3.6	65	5.62	.005
Weak Republicans	−1.50	3.1	4	−5.27	4.0	97	1.85	.025
Medium Republicans	0.00	5.2	16	−4.30	4.1	147	3.88	.005
Strong Republicans	−3.67	7.5	3	−6.24	3.1	116	1.31	ns[b]

[a] Combines voters for Nixon and Wallace.
[b] $p > .05$.

Table 2b. Response Consistency, Party Identification and the Vote: Expectation of Policy Satisfaction Index[a]

Party Identification	Voters for Humphrey			Voters Against Humphrey			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
Strong Democrats	3.44	3.5	188	0.27	4.3	34	4.62	.005
Medium Democrats	1.71	3.3	139	−1.02	2.5	102	6.99	.005
Weak Democrats	2.24	3.3	49	−0.31	3.1	44	3.81	.005
Independents	1.48	2.7	20	−0.87	2.1	65	4.02	.005
Weak Republicans	−0.88	1.4	4	−1.64	2.9	97	0.51	ns
Medium Republicans	2.19	3.4	16	−1.38	3.1	147	4.33	.005
Strong Republicans	−0.17	3.9	3	−1.71	3.3	116	0.78	ns

[a] See footnotes to Table 2a.

who voted for Nixon are not significantly more prone to deprecate their party consistently than are Strong Democrats who share their foreign policy beliefs but who did not defect. These Democrats are on the average less pro-Democratic, but their scores are so variable as to make the mean an unreliable estimate of the response tendencies within the group. According to cognitive balance theories, this group of voters should have the greatest psychological incentive for rationalization; their failure to conform to its predictions is a source of doubt about the rationalization interpretation.

Our doubts increase when we consider that those Strong Democrats who do not distinguish the parties on their war-handling capability and who voted against their partisanship in this instance behaved contrary to their overall response tendency and passed up an opportunity to deprecate the performance of the Democratic Administration.

With such small N's within cells, these findings can hardly be considered conclusive. They should give pause to those who argue the rationalization of defection.

These analyses partly clear the Key and Boyd approaches of the problem of rationalization. If we are satisfied on this score, we have, especially in the normal vote approach, techniques for establishing whether or not there is a correlation between policy preferences and voting choices in the electorate at large. But, as Boyd explicitly acknowledges, this does not permit us to conclude "that the issue was causally related to the vote or that the issue made some net contribution to the observed Democratic vote."[18] Since it is necessary to establish precisely these facts in order to demonstrate that policy voting has taken place, we are not fully content with normal vote analysis as a technique.

While agreeing with Boyd's analysis of the shortcomings of the normal vote approach—and, incidentally, with his analysis of its strengths, as well—we have reservations about his contention that these shortcomings are inherent in survey data.[19] We believe the difficulty lies with aggregating across the electorate, rather than with modeling the individual voting decision, and with treating each policy issue in

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[18] Richard Boyd, "Popular Control . . ." p. 432.
[19] Richard Boyd, "Popular Control . . ." p. 432.

Table 3. Response Consistency of Democratic Identifiers By Strength of Partisanship, Presidential Vote, and Beliefs about Which Party Can Better Handle the Problem of War: Partisan Issue Alignment Index

Which Party Can Handle War/ Partisan Strength	Voters for Humphrey			Voters for Nixon[a]			t	Statistical Significance
	Mean	S.D.	N	Mean	S.D.	N		
"Democrats"								
Strong Democrats	6.57	3.6	67	3.50	5.9	4	1.56	ns[b]
Medium Democrats	5.96	3.5	26	0.00	—	1	—[c]	—
Weak Democrats	6.50	2.2	14	−8.00	—	1	—[c]	—
"Same by Both"								
Strong Democrats	4.06	4.0	87	−0.17	2.9	6	2.53	.01
Medium Democrats	1.26	4.1	85	−3.11	3.5	36	5.49	.005
Weak Democrats	2.11	4.4	27	−3.06	2.8	17	4.19	.005
"Republicans"								
Strong Democrats	−1.88	4.9	8	−5.17	5.4	6	1.10	ns[b]
Medium Democrats	−2.90	4.2	10	−5.70	4.1	23	1.73	.05
Weak Democrats	8.00	—	1	−3.71	4.1	7	—[c]	—

[a] Since the "Republicans" was a specific category in the item, 56 Wallace voters are not considered here.
[b] p>.05.
[c] Too few cases for comparison.

isolation from every other. Taking the issues one at a time means that the approach is continually reapportioning the same observed variation in the vote. In this way many factors can appear as indispensable in explaining the vote. Given the multifaceted nature of political reality, disciplined statements about the causes of behavior will be necessarily multivariate.

There is no way to avoid modeling the individual voting decision and still estimate policy voting. The analysis must bring the respondents by means of their own testimony into a direct relationship with the issues. The analyst must determine that the voter viewed the candidates or parties in relation to the policy issues. In short, the analyst has to ascertain whether the voter has satisfied the necessary and sufficient conditions for policy voting.

The Requirements of Policy Voting

We have from diverse sources more or less consonant lists of the elements required for policy voting.[20] The elements of such lists describe a decision process which includes as independent variables: (1) The salience of the policy issue (or issue area) for the voter. Butler and Stokes, term it a "link" between the issue and the self.[21] Davis, Hinich, and Ordeshook make attention to the issue and responsiveness to the candidates a requirement.[22] (2) The voter's position on the issue or his preferences for a line of policy. (3) The voter's perception of the candidates' or parties' position on the policy issue.[23]

Given these elements, an influence of policy on voting is said to be present when the voter's comparative evaluation of the candidates or his vote or both are independently affected by the relative proximity of the candidates to his position on issues that are for him salient. Figure 2 idealizes this decision process as a recursive arrow diagram. It reflects the assumption that *ceteris paribus* the voter will prefer (i.e., evaluate more positively), the candidate closer to him on salient issues and that he will, if he votes, vote for the preferred candidate.

Assessing the Requirements of Policy Voting

From the perspective of assessment, this model gives rise to the need for measures of the salience of issues and of the perceived position of the candidates on the issues.

"Salience," which can be measured as RePass suggests by respondents' identification of the "most important problems the government . . . should try to take care of,"[24] is an indication of an *affective* link between the voter and the

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[20] See: Butler and Stokes, *Political Change . . .*, "The Analysis of Issues," pp. 173–92 and Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June, 1970), pp. 426–428.

[21] David Butler and Donald Stokes, *Political Change . . .*, p. 174.

[22] Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . .," p. 429.

[23] Those familiar with the Davis et al., "An Expository Development . . ." article, will recognize that in singling out the policy or issue component of these elements, we have made policy voting a special case of their broader class of 'rational voting."

[24] See: Items 16 through 16p on the SRC/ICPR 1968 Pre-Election Questionnaire.

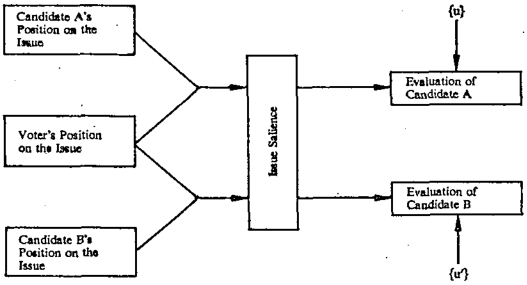
Candidate A's Position on the Issue
Voter's Position on the Issue
Candidate B's Position on the Issue
Issue Salience
{u}
Evaluation of Candidate A
Evaluation of Candidate B
{u'}

Figure 2. Necessary and sufficient conditions for policy voting.

issue. Salience does not, however, ensure a *cognitive* link. Simply because voters are attracted to an issue or think it an important problem is no guarantee that they will necessarily seek or retain information about it. If this generalization needs support, consider, for example, the fact that in 1967 more than one-third of those who thought the war in Vietnam important enough to engage in the unusual political act of trying to change someone's opinion on the problem could not identify Hanoi as the capital of North Vietnam.[25]

Partly because of this lack of cognitive fidelity, we feel it is essential to measure the voter's perception of the candidates' issue positions. Davis, Hinich, and Ordeshook assume that *"all citizens make identical estimates"* of a candidate's issue position.[26] However expedient such an assumption may be in modeling candidate behavior, it is not necessary in order to model voting behavior. Singularity of perception of the candidates' stands is, of course, counterindicated by an impressive amount of data.[27] But the assumption is not required on analytic grounds either.

Relaxing the assumption that voters share a common perception of where the candidates stand in "issue space" entails admitting causes of that perception beside or in addition to candidate behavior. Once it is established where the candidates are *perceived* to stand in issue space, the voter's individual loss function[28] or other psychological distance measure[29] can be computed and related to his voting behavior. In other words, the basic hypothesis that the voter will vote for the candidate most proximal to him on the issues is common to both approaches. The hypothesis does not depend upon the candidate's being perceived at one and only one point.

As formulated, the hypothesis is undoubtedly true. It is true of the electorate on particular issues, and it is true when issues are aggregated and a composite index is generated.

Consider Figure 3, for example: In this graph the abscissa is an attenuated version of a scale measuring the signed difference of the perceived distance from the voter to Humphrey and the voter to Nixon, on a seven-point "Vietnam policy" scale.[30] Using the "normal vote" component as our null model, we find that

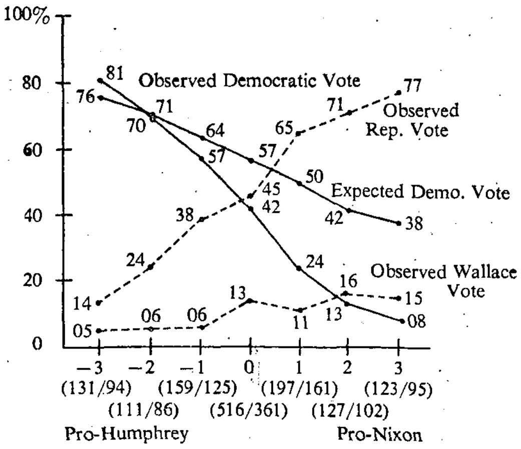

Figure 3. Vietnam issue distance and the vote in 1968.

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[25] Richard A. Brody and Sidney Verba, "Hawk and Dove: The Search for an Explanation of Vietnam Policy Preferences," *Acta Politica*, forthcoming.

[26] Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . .," p. 431, emphasis in the original.

[27] See, for example, Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954) and Benjamin I. Page and Richard A. Brody, "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review*, 66 (September, 1972), forthcoming.

[28] Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development . . .," p. 432.

[29] The 1968 election was the occasion for at least three independent efforts to relate psychological distance/proximity to voting behavior. Michael Shapiro's study of voters in Hawaii is reported in "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*. 63 (December, 1969), 1106–1119. The two based on national samples are represented in preliminary reports in convention papers: See: Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, The Urban Crisis, and the 1968 Presidential Election: A Preliminary Analysis," paper delivered at the Annual Meeting of the American Sociological Association, San Francisco, California, September, 1969; and David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party, Ideology, Issues, and Candidate Choice: A Preliminary Theory-Relevant Analysis of the 1968 American Presidential Election," paper delivered at the Eighth World Congress, International Political Science Association, Munich, Germany, August 31-September 5, 1970.

[30] See: SRC/ICPR, 1968 Post-Election Questionnaire, questions 67, 67b, 67c, and 67e.

there is a long-term effect which reflects the correlation of the Vietnam distance comparison with partisan identification. An even larger short-term component can also be seen in these data;[31] proximity on this issue and voting behavior clearly covary.

Similarly, in Figure 4, we can see that a composite measure of distances across salient issue areas[32] shows substantial long and short-term components. A long-term effect of this magnitude (L = 14.5) would rank first among the fifteen issues examined by Boyd. A short-term effect of this magnitude (S = 8.37) would rank just below the second most potent of Boyd's issues. These data unambiguously show that issue proximities covary with voting behavior.

A Question of What Causes What

If it is true, as we have shown, that voters vote for the candidates proximal to them on the issues, then the processes relevant to the perception of that proximity become pertinent in assessing the extent of policy voting. These processes are pertinent because we may wish to exclude from our definition of policy voting some behaviors which they describe.

Consider the elements of the model depicted in Figure 2: If we relax the assumption of recursivity and permit the voter's evaluations of the candidates (or his vote) to cause his perceptions of their stands or his own policy position or both, we can distinguish three "proximity" processes. These processes, singly or in combination, could produce graphs like those in Figure 3 or 4.

The first process, "Policy Oriented Evaluation," is depicted in Figure 2. It describes the cases in which the prior held policy position of the voter and his perceptions of the candidates' positions (which are presumably independent of each other) *cause* his evaluation. Thus, in the slice of time considered by this model or covered in a cross-sectional survey, the voter's position and perception are invariant, and evaluations vary in accord with the issue proximities.

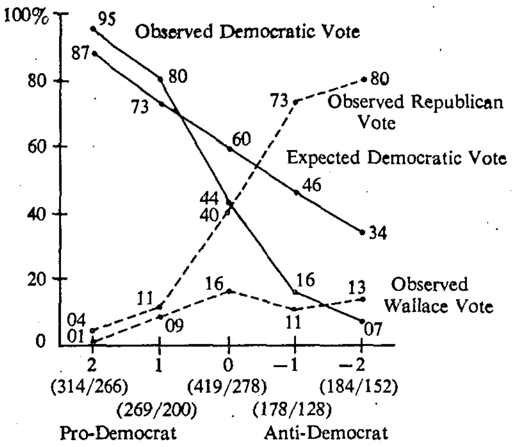

Figure 4. Issue alignment and the vote in 1968: Salient issue distances.

Suppose we change the invariant elements of this model: If perceptions and evaluations were invariant and the respondent's own position yielded to or formed around the position he believed was held by the candidate he favored (or intended to vote for), we would say that the voter had been "persuaded" by the candidate. "Persuasion" may be a perfectly legitimate political process, but in the slice of time being considered here, it is an effect of, not a cause of, candidate evaluation. In these terms, we cannot say that the voter who is persuaded is evaluating or voting on the basis of policy.

The third process stems from allowing the perceived position of the candidate to vary with the voter's own position and his evaluation of the candidate. Under this model, a voter could see a candidate as close to himself on an issue because he otherwise felt positive about the candidate, and he could see other candidates as distant from him on the issue(s) because he felt negative about them for other reasons. In other words, a voter would "project" his own (or a nearby) stand onto candidates he favored. We are reluctant to call those who project "policy voters."

Both persuasion and projection have been found to operate in the American electorate. Berelson, Lazarsfeld, and McPhee, found these processes operating on policy stands as clear as President Truman's position on Taft-Hartley repeal[33]; we found them operating on the less clear Vietnam policy positions of candidates Nixon and Humphrey in 1968.[34]

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[31] Compared with the fifteen issues examined by Richard Boyd ("Popular Control . . .," Appendix II), comparative distance on Vietnam policy, with L = 7.91 and S = 8.65, ranks fifth on long-term effect and third on its short-term component.

[32] Our composite measure is the Partisan Issue Alignment Index (see Footnote 15). We interpret the response to "which party do you think would be most likely to do what you want on this . . .?" as a dichotomous (near, not near) distance measure. In this graph, the abscissa values are a grouping of the +9 to −9 index; −9 to −6 = −2; −5 to −1 = −1; 0 = 0; 1 to 5 = 1; and 6 to 9 = 2.

[33] Bernard Berelson, Paul Lazarsfeld, William McPhee, *Voting*, pp, 220–222.

[34] Benjamin I. Page and Richard A. Brody, "Policy Voting. . . ."

The presence of these two "alternate" processes in the electoral system makes it inappropriate to declare policy-oriented evaluation *the* cause of the correspondence between issue proximity and voting behavior. We need some means for examining the potential for "persuasion" and for "projection" and of estimating them as separate processes.[35]

If we confine ourselves to the "position," "perception," and "evaluation" variables in a given survey, there is no way to estimate the separate effect of each of the three proximity processes. The solution to the problem lies with panel data within a given election year, in the manner of the Lazarsfeld studies, or with expanding the set of variables considered. This latter approach would entail finding causes of persuasion which are not also causes of projection or policy-oriented evaluation; finding causes of projection which are not causes of either persuasion or policy-oriented evaluation; and finding causes of policy-oriented evaluation which are not causes of the other proximity processes.[36] The specification of such causal factors would permit us to use a technique of causal modeling which explicitly takes account of the possibility of all three processes working and estimates the extent of each; we refer, of course, to the econometrician's simultaneous equation estimation procedures.

In the case of policy-oriented evaluation of a candidate, the simultaneous equation approach suggests a three-equation model; in this model, each of the central variables (own position, perception of candidates' positions, and evaluation of the candidate) has a single equation in which it is the dependent variable and hence a function of the other two. This would give us one equation for each proximity process. Up to this point without the independent (exogenous) causal factors, no estimates can be derived; once the independent causal factors are found, we can at last achieve an estimate of the extent of policy voting.[37]

The discovery of these independent causal factors is no easy matter. There is no statistical procedure which will uncover them. They must come from our theories of behavior and our knowledge about the act of voting. However difficult it is to specify such causal factors, that is exactly where the problem is. If the estimation of policy voting is important to the understanding of the role of the citizen in a democracy—and theorists of democracy certainly write as if it is—then any procedure which fails to control for projection and persuasion will be an undependable base upon which to build our understanding.

Research such as that of Boyd, Key, Kovenock, Jackson, Pomper, RePass, and Shapiro—and, we hope, our work, too—should unsettle those who are complacent in their belief that policy and issues play no role in the electoral decisions of the American voter. Having unsettled the complacent, let us take the steps necessary to settle the question.

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[35] David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party . . . ," pp. 29–31, discuss this problem and refer to reasons for believing it not to be important. Their reasoning is based on the number of issues examined, the nature of their data gathering procedures, the fact that the items most clearly associated with the several candidates are different for each of them, and the fact that on some issues those who vote for one candidate are closer to another. Such plausible evidence encourages rather than satisfies the search for estimates of the three proximity processes.

[36] Those acquainted with econometric approaches to nonrecursive causal modeling will recognize these causal sets as the exogenous variables required to "identify" a system of simultaneous equations. See, for example, Franklin Fisher, "Statistical Identifiability," in *International Encyclopedia of the Social Sciences, Volume 15*, ed. David Sills, (New York: The Macmillan Co. and Free Press, 1968), pp. 201–206.

[37] For example, see: Michael Shapiro, "Rational Political Man . . ." and John Jackson, "The Importance of Issues and Issue Importance in Presidential Elections," Discussion Paper #3, Program of Quantitative Analysis in Political Science, Government Department, Harvard University, January, 1972.

Comment: The Issues in Issue Voting*

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Issue voting, it appears, has been rediscovered once again. The articles by Professors Boyd and Pomper join a rapidly growing literature. Some thirty books, articles, and convention papers[1] have appeared in recent years, all suggesting that issues, in one way or another, have had consequences with respect to presidential voting choice.

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[*] I should like to thank my colleagues Aage R. Clausen, C. Richard Hofstetter, and Theodore W. Meckstroth for very helpful reactions to an earlier draft of this comment.

[1] The literature on this topic includes: Richard W. Boyd, "Presidential Elections: An Explanation of Voting Defection," *American Political Science Review,* 63 (June, 1969), pp. 498–514; Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review,* 66 (June, 1972), pp. 429–449; Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, the Urban Crisis, and the 1968 Presidential Election: A Preliminary Analysis," a paper prepared for the 1969 meeting of the American Sociological Association, September, 1969; Richard A. Brody and Benjamin I. Page, "Policy Voting and the Electoral Process: The Vietnam War Issue," a paper prepared for delivery at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7–11, 1971; Richard A. Brody and Benjamin I. Page, "Comment: The Assessment of Policy Voting," *American Political Science Review,* 66 (June, 1972), pp. 450–458; Comparative State Election Project, *Explaining the Vote: Presidential Choices in the Nation and the States, 1968* (Chapel Hill, N.C.: Univ. of North Carolina Press, 1972); Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review,* 59 (June, 1965), pp. 321–336; Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review,* 63 (December, 1969), pp. 1083–1105; John Osgood Field and Ronald E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly,* 33 (Fall, 1969), pp. 380–398; Martin Fishbein and Fred S. Coombs, "Modern Attitude Theory and the Explanation of Voting Choice," a paper prepared for delivery at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7–11, 1971; William H. Flanigan, *Political Behavior of the American Electorate,* 2nd ed. (Boston: Allyn and Bacon, 1972); Carl C. Hetrick, "Issues and Politics: An Exploration in Policy-Motivated Political Behavior," a paper prepared for the 1968 Annual Meeting of the American Political Science Association, Washington, D.C., September 2–7, 1968); John H. Kessel, *The Goldwater Coalition: Republican Strategies in 1964* (Indianapolis: Bobbs-Merrill, 1968); John H. Kessel, "Pennsylvania, 1968: Analysis of a Belief System," a paper prepared for delivery at the 1971 Annual Meeting of the Midwest Political Science Association, April 29-May 1, 1971, Chicago, Illinois; V. O. Key, Jr. (with the assistance of Milton C. Cummings, Jr.), *The Responsible Electorate: Rationality in Presidential Voting, 1936–1960* (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1966); Samuel A. Kirkpatrick, "Political Attitudes and Behavior: Some Consequences of Attitudinal Ordering," *Midwest Journal of Political Science,* 14 (February, 1970), pp. 1–24; Samuel A. Kirkpatrick, "Political Attitude Structure and Component Change," *Public Opinion Quarterly,* 34 (Fall, 1970), pp. 403–407; David M. Kovenock, Philip L. Beardsley, and James W. Prothro, "Status, Party, Ideology, and Candidate Choice: A Preliminary Theory-Relevant Analysis of the 1968 American Presidential Election," a working paper prepared for the Eighth World Congress of the International Political Science Association, Munich, Germany, August 31-September 5, 1970; Richard M. Merelman, "Electoral Instability and the American Party System," *The Journal of Politics,* 32 (February, 1970), pp. 115–139; Peter B. Natchez and Irvin C. Bupp, "Candidates, Issues and Voters," *Public Policy,* 1968, pp. 409–437; John C. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science,* 14 (February, 1970), pp. 25–42; Gerald M. Pomper, "After Twenty Years: The Report of the APSA Committee on Political Parties," a paper prepared for delivery at the 1970 Annual Meeting of the American Political Science Association, Los Angeles, California, September 8–12, 1970; Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review,* 66 (June, 1972), pp. 415–428; David E. RePass, "Issue Salience and Party Choice," *American Political Science Review,* 65 (June, 1971), pp. 389–400; John P. Robinson, "Public Reaction to Political Protest: Chicago, 1968," *Public Opinion Quarterly,* 34 (Spring, 1970), pp. 1–9; Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review,* 63 (December, 1969), pp. 1106–1119; J. Merrill Shanks, "The Quality of Electoral Change, 1952–1964," a paper prepared for delivery at the 1969 Annual Meeting of the American Political Science Association, New York, N.Y., September 2–6, 1969; Peter W. Sperlich, *Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior* (Chicago: Rand-McNally, 1971); Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review,* 60 (March, 1966), pp. 19–28; Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluations," *American Political Science Review,* 64 (December, 1970), pp. 1167–1185; Herbert F. Weisberg and Jerrold G. Rusk, "Perceptions of Presidential Candidates: A Midterm Report," a paper presented at the 1971 Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7–11, 1971.

There are reasons for all this intellectual effort. Some attend any scientific enterprise—researchers working independently who come to similar conclusions; the redundancy necessary to communicate through a "noisy channel" to a busy professional audience—but reasons can also be found in the complexity of the subject matter. The inquiry does not take the form of a simple debate over whether issues are or are not consequential. The proper questions are: (1) How important are attitudes about issues among several factors that motivate a person to vote for one candidate rather than another? (2) How important is any given issue within the set of issues that does affect the voting decision? (3) Under what specific conditions is issue voting, thus defined, enhanced or depressed? The answers one obtains to these questions, in turn, depend on measurement considerations: (1) To what degree are issues reflected in the data set? (2) What kind of measures of the importance of issues are employed in the analysis? (3) What is the "true level" (assuming no measurement error) of issue voting among the particular population from which the data has been collected?

In their articles, Richard Boyd and Gerald Pomper also join those[2] who have asserted that *The American Voter*[3] needs to be updated with respect to issue voting. The portrait of an electorate that has little concern for issues, they tell us, is no longer valid. Even granting that a twelve-year-old book probably is in need of revision, this claim adds two more questions to the list we already have: (1) What did *The American Voter* actually say about the extent of issue voting? (2) How much change has taken place in the "true level" of issue voting from the 1950s to the present? In sum, we must deal with several interrelated considerations to get any clear idea of how much revision is needed in the conclusions presented in *The American Voter* about the extent of issue voting. Let us look at each of these in turn.

What did "The American Voter" say?

The statements by Professors Boyd and Pomper are standard interpretations of the received doctrine. Most of us, myself included, have had such statements in our lecture notes ever since *The American Voter* appeared. It is important to remember, however, that three *different* analyses of issue voting were made in the book. The first, cited by both Boyd and Pomper, sets up three tests for "issue-oriented partisan behavior"—expression of an opinion, perception of what the government is doing, and perception of party differences. The book states that between 18 per cent and 36 per cent of the respondents pass these tests, depending on the issue involved.[4]

The second analysis was based on the measure the authors developed for levels of conceptualization; they reported that only 3½ per cent of the voters fell into the "full ideologue" category, but went on to say that "these are the people who best fit the assumptions about 'political man.' "[5] A small segment of the population, in other words, but one capable of playing a crucial and ideologically motivated role in deciding an election.

The third analysis is the six-component model of individual voting choice. Here, if one looks at the standardized regression coefficients[6] as measures of the importance of the components, he discovers that issues were more important in 1952 than in 1956. In 1952, domestic issues ranked second among the six components, while attitudes on foreign issues were more important than were attitudes toward the Democratic nominee, Adlai Stevenson. In short, *The American Voter* presented a mixed verdict on the importance of issues. Thus, frequent statements that the book suggested issues were not of great consequence say more about the loss of information when a complex phenomenon is reduced to a dichotomous variable (important/unimportant) than they do about the content of the book itself.

The Nature of the Data Set

Four different sets of questions have been used for the analysis of issue voting; each has characteristics which affect the results of the analysis. The first are the precoded questions used by Professors Boyd and Pomper. These questions facilitate comparisons between issues and make longitudinal comparisons possible for repeated items. They do not, however, tell anything about the importance of issues as compared to parties, candidates, or respondents' life circumstances. Nor can they capture references to issues other than the finite set about which questions are asked.

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[2] Field and Anderson, "Ideology in the 1964 Election;" Kovenock, Beardsley and Prothro, "Status, Party, Ideology, and Candidate Choice;" Natchez and Bupp, "Candidates, Issues and Voters;" Pierce, "Changing Role of Ideology;" and RePass, "Issue Salience and Party Choice."

[3] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960).

[4] Campbell et al., *American Voter*, chap. 8.

[5] Campbell et al., *American Voter*, chap. 10.

[6] Donald E. Stokes, Angus Campbell, and Warren E. Miller, "Components of Electoral Decision," *American Political Science Review*, 52 (June, 1958), pp. 367–387.

The second type of question that has been used is the open-ended question about parties and candidates. These standard queries—Is there anything in particular that you like/don't like about the Democratic/Republican party? Is there anything in particular about the Democratic/Republican candidate that might make you want to vote for/against him?—elicit a certain amount of material about issues, allow analysis along several important cognitive dimensions, and certainly provide our most comprehensive longitudinal data set. But since these questions do not ask specifically about issues, they stimulate fewer issue responses than comments about candidates and parties. Therefore analyses based upon this data set tend to underestimate the effects of issues.

The third data set was added to the Survey Research Center interview schedule in 1960. The key question in this open-ended sequence was: "What would you personally feel are the most important problems the government should try to take care of when the new President and Congress take office in January?" The strengths and weaknesses of of this question lie in the focus on issues. It elicits richly detailed issue data. It opens real possibilities for comparisons between issues, but inhibits comparisons of the importance of issues vis-à-vis attitudes on other topics.

The fourth data set consists of precoded questions about the respondent's stand on given issues and his perceptions of the candidates' positions on the same issues. These data make it possible to calculate attitudinal proximity between the respondent and the candidates. These items were first used in the Brody-Page-Verba study and the Comparative State Election Project in 1968, and were added to the election study of what is now called the Center for Political Studies in 1970. These relatively new attitudinal proximity scores show attitudes about issues to be more important than was evident when our estimates were based on the respondent's position alone.

I do not wish to overstate the differences among the four data sets. A careful analyst will be able to discern effects of the issues by using any one of them; if he is sensitive to the differences among the data sets, he can move from one to another in the course of his analysis. Since the responses contain greater or lesser amounts of issue data, however, they do affect one's findings about the importance of issues as compared to other factors.

The Nature of the Analysis

What one does with the data after they have been collected will also affect one's measures of the importance of the issues. In general, two lines of the analysis have been developed in the recent literature on issue voting: One group of scholars has concentrated on those voters who are particularly likely to be sensitive to issues. This would include Natchez and Bupp's[7] work on issue publics, Hetrick's[8] on policy-motivated voters, and Field and Anderson's[9] on ideologues. All isolated those voters whose responses to open-ended questions showed a concern with the issues. Other scholars perfected new indices or used new concepts such as Converse's[10] normal vote to monitor the effect of issues. John Pierce[11] suggested measures for "informational ideologues" and "affective ideologues" to add to the "conceptual ideologues" who appeared in *The American Voter*. Richard Boyd[12] employed indices of party compatibility, candidate compatibility, and issue compatibility to analyze vote defection. David RePass[13] worked out a fine measure of issue partisanship. Merrill Shanks[14] and Richard Boyd[15] have shown new ways in which Converse's normal vote measure could be employed. Both of these lines of analysis—the isolation of issue-motivated voters and the use of issue-sensitive measures—have naturally tended to heighten our awareness of issue voting.

Change in the Level of Issue Voting

Assuming now that all these measurement questions have been solved and that we have satisfactory evidence of a change in the level of issue voting, what could account for this change? There are at least three possibilities. First, attitudes about issues may have become relatively more important as compared with other attitudes in the electorate as a whole. Second, the proportion of voters to whom attitudes about issues are relatively important may have grown sufficiently to be reflected in the overall figures. The third possibility is that the voters are no more and no less aware of issues than they ever have been, but instead the parties may have moved farther apart on some ideological dimensions so that voters are more likely to perceive the issue consequences of their votes. The difficulty, of course, is that these processes are not mutually exclusive. All three of them may have been going on at the same time, in addition to the methodological innovation that has also been taking place. Hence the problem is to unravel multiple causes for change in the observed level of issue voting.

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[7] Natchez and Bupp, "Candidates, Issues and Voters."

[8] Hetrick, "Issues and Politics."

[9] Field and Anderson, "Ideology in the 1964 Election."

[10] Philip E. Converse, "The Concept of a Normal Vote," in *Elections and the Political Order*, ed. Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, (New York: Wiley, 1966), chap. 2.

[11] Pierce, Changing Role of Ideology."

[12] Boyd, "Voting Defection."

[13] RePass, "Issue Salience and Party Choice."

[14] Shanks, "Quality of Electoral Change."

[15] Boyd, "Normal Vote Analysis of 1968 Election."

Elementary prudence would suggest analytical procedures that separate substantive and methodological causes for such change. The substantive argument is that there have been changes in the real world of American politics, that the 1950s were quiescent in contrast to the turbulent 1960s and/or that the relatively younger, relatively better educated electorate we now have is more aware of issues. To demonstrate this type of change, one should use the same analytical technique with the same data set over a series of elections. The methodological arguments hold that the earlier data sets, or techniques of exploiting them, were not as sensitive to issues as are some newer data sets or techniques. To show this, one should compare the two data sets with the same technique in the same election, or compare the two analytical procedures with the same data set in the same election. If one wishes to make both a methodological and a substantive argument, he should first make this methodological case, then proceed to longitudinal analysis. If he wishes, in addition, to argue that *The American Voter* is inadequate, he must anchor the beginning of his time series in the data sets and measures in that book.

A Closer Look at Some Specific Problems

The unhappy fact is that the methodological problems are of such a character, and the number of comparable data sets is so limited, that even the most careful scholars are unable to avoid pitfalls. Pomper's and Boyd's analyses are quite different. Pomper's is a longitudinal analysis which has the great virtue of joining Stokes,[16] Field and Anderson,[17] and Pierce[18] in presenting comparable data over time. Boyd's cross-sectional analysis of the 1968 election employs a normal vote analysis to discriminate between long-term and short-term effects. Both analyses, however, have important limitations. Pomper's work was hampered by the limited number of precoded questions that had been asked over time, and by the nature of these items. Boyd's application of the normal vote is inhibited because these calculations produce estimates for a Democratic candidate running against a Republican candidate, and 1968 was a year in which three candidates were in the race.

Leaving aside the question of foreign aid (something of a special case and not central to his analysis), Gerald Pomper presents evidence that there is an increasing correlation between party identification and a "liberal" position on five issues (aid to education, medical care, job guarantee, fair employment, and school integration) as well as an increasing awareness of party differences and an increasing consensus as to how the parties differ. He has done what he could with the data available to him. He has exhausted the set of questions that were asked over time, and he has exploited the follow-up questions about the parties. But to judge the adequacy of his analysis, one must note the wording of the questions themselves. The 1956 item on aid to education read:

> If cities and towns around the country need help to build more schools, the government in Washington ought to give them the money they need. Do you have an opinion on this or not? (IF YES) Do you agree that the government should do this or do you think the government should not do it?

The wording of the questions varied slightly from issue to issue, and a slightly different form was used in 1964 and 1968, but each of these items asked about an increase in the power of the federal government. Now it happens that attitudes on this issue—the desirability of increasing the power of the federal government—are also strongly related to party identification. In fact, the zero-order relationships between attitudes on the power of the federal government and party identification are stronger than those on the five specific issues reported by Professor Pomper. This suggests that the original relationships reported stem from a combination of two attitudes: that about the specific issue and that about the federal government. Table 1 presents the results of a test to determine how much of the relationship was caused by the association between party identification and attitudes about the federal government. Kendall's Tau-b measure of association has been calculated for the relationships reported by Pomper, as have the first-order partials in which the relationship has been

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[16] Stokes, "Dynamic Elements of Contests for Presidency."

[17] Field and Anderson, "Ideology in the 1964 Election."

[18] Pierce, "Changing Role of Ideology."

Table 1. Reanalysis of Pomper's Data*

Issue	Kendall's Tau-b			
	1964		1968	
	No Control	Control	No Control	Control
Aid to Education	.224	.132	.233	.163
Medicare	.311	.216	.269	.216
Job Guarantee	.218	.154	.175	.122
F.E.P.C.	.157	.063	.160	.116
School Integration	.063	−.027	.141	.080

* Kendall's Tau-b measures of association between specific issues and party identification, with and without control for respondent's attitude toward increasing power of federal government.

controlled for the respondent's attitude toward increasing the power of the federal government. When this control is introduced, a different pattern is evident. With the social welfare issues, half of the variance purportedly accounted for by the original relationship is eliminated; for the two civil rights items, the reduction in explanatory power is even greater. Much of the temporal variation is eliminated in three social welfare items, but it is even more pronounced with the integration questions. It seems clear that we are dealing with a covariation of two attitudes and that we must untangle the effects to find out what has been happening. But such analysis is blocked by another limitation of the data set: The question that asked directly about federal power was not included prior to 1964. Hence we know only that the single dimension of federal power affects the relationship in 1964 and 1968. We are limited to inference and speculation about 1956 and 1960.

Since Professor Pomper's analysis depends so heavily on attitudes about the power of the federal government, I wish this dependence had been stressed in his narration. There is literary convenience in using the single words "liberal" and "conservative," but there is also distortion. It requires longer phrases to refer to Democratic emphasis on Washington, D.C., and Republican emphasis on state capitols, but these references capture one of the most important partisan distinctions of our era. I also think the analysis loses force by reporting the voters' perceptions of the parties as "liberal" and "conservative." Just as the Lokele drummers of the Congo refused to believe that Europeans did not communicate by drums, so many political scientists seem to insist that most Ameicans use the concepts "liberal" and "conservative" in spite of abundant evidence that these terms are too abstract to be grasped by people of average education. In fact, the ability to discern which party "is closer to what you want" (the point requested in the original interview item) is more vital for voter rationality than a capacity to understand the denotation of abstract concepts.

Although Professor Pomper's finding is limited, the limiting derives from the nature of the data set rather than from lack of skill in exploiting the data. Indeed, he has greatly strengthened his study by introducing controls for age, education, and race/region. Although changed perceptions of party differences have been most apt to occur among blacks and Southern whites, Pomper's analysis permits the general conclusion that the increased sensitivity to issues is characteristic of the electorate as a whole. This, of course, is a different kind of electoral shift from one that occurs because of changes in the demographic structure of the electorate.

Turning to Boyd, we note that his analysis employs one of the most fruitful ideas to come out of the voting studies, Philip Converse's concept of the normal vote. The question is whether the real-world situation to which Boyd applies this concept is sufficiently similar to that from which the concept was abstracted. The normal vote assumes a contest in which one Republican is running against one Democrat. Professor Boyd properly calls attention to the difficult challenge posed to such normal vote analysis by Wallace's candidacy in 1968, a year in which three candidates were able to win votes in the Electoral College.

Boyd handles this difficulty by assuming that there is one Republican (Richard Nixon), one Democrat (Hubert Humphrey), and one "short-term force" (George Wallace). An equally plausible assumption is that the normal Democratic vote is split between the non-Southern and Southern Democratic candidates. This simplification is reasonably consistent with the facts. Wallace's votes came principally from Independents (half of whose votes are assigned to the Democratic candidate by the normal vote calculations) and from Democrats. And the normal Democratic vote gives a good estimate of the combined Humphrey-Wallace vote. To take the statistics on the pace of progress toward civil rights from Boyd's Figure 1, for example, the normal Democratic vote is 68 per cent among those who think civil rights is going too slowly, 62 per cent among those who think the speed is about right, and 54 per cent among

Table 2. Reanalysis of Boyd's Data*

Issue	Short-Term Effect Scores Assuming Norm. Dem. Vote to Humphrey Only	Short-Term Effect Scores Assuming Norm. Dem. Vote to Humphrey +Wallace
Aid to Education	7.03	2.25
Mass Demonstrations	3.18	1.43
Escalation-De-escalation	7.21	1.27
Speed of Civil Rights	6.16	.92

* Short-term effect scores based on assumption that Humphrey was sole recipient of normal Democratic vote and on assumption that normal Democratic vote was split between Humphrey and Wallace.

those who think things are moving too fast. The combined Humphrey-Wallace votes for these three groupings are 70 per cent, 56 per cent, and 49 per cent, respectively. The estimates are rather close, and the deviations—+2, −6, and −5, respectively—accord with suspicions about the probable impact of the civil rights issue.

If we assume that the normal Democratic vote was split between Vice President Humphrey and Governor Wallace, we get a very different picture of the impact of the issues. Table 2 presents the results of a recalculation of Professor Boyd's "short-term component" measure. His figures are compared with those obtained by assuming that Humphrey and Wallace split the normal Democratic vote. The table presents these data for one issue from each of Boyd's four issue areas: social welfare, mass protest, Vietnam, and civil rights. One gets a very different estimate of the average magnitude of the issue effects, and by inference, a different notion of how important issues were when compared with other possible motivations. Changing the assumption about what happened to the normal Democratic vote also leads to a different ordering of the importance of the issues with respect to each other.

The problems we have been discussing relate to Boyd's estimates of short-term effects in 1968. They do not handicap his normal vote analysis of the escalation/de-escalation issue in 1964. Nor do they hamper in any way his ingenious computation of a long-term component associated with various issues (also presented in Appendix II). It is most useful to be able to measure the extent to which issues are or are not associated with traditional partisan divisions.

This leads to a paradox: The normal vote was conceived in order to be able to distinguish the *short-term* forces operating in a particular election; yet its best 1968 application resulted in a good measure of the *long-term* partisanship associated with given issues. One reason for this seeming inconsistency—the three-man race in 1968—is fairly obvious. Another is less apparent. It has to do with two ways of understanding the normal vote. One is the *conceptual distinction* between long-term and short-term forces. The other is the *operational selection* of party identification to represent the long-term forces. If long-term forces are to be represented by an attitude, then party identification is the prime candidate. This choice is clear enough, but the temporal distinction then implies that all other attitudes, may be considered as short-term stimuli. This inference is dubious. Attitudes concerning candidates who are not renominated are important only in single elections. Other attitudes, such as those about renominated candidates and issues that are salient for perhaps a decade, affect the outcome of two or three consecutive elections. Still other attitudes, such as those concerning economic policy and the desired locus of governmental power, have influenced voting choices for much more than a generation. The normal vote calculations control for the effects of party identification, the single most important long-term influence. This is essential. Still, we should be mindful that what is left is an admixture of short-term, medium-term, and long-term influences.

Distinguishing between attitudes on the basis of whether the self is an object is equally useful and more consistent. The party identification question—Generally speaking, do you consider yourself a Republican, a Democrat, an Independent, or what?—invokes both the self and a reference group. Since both the self and a salient group are objects, party identification should be regarded as a central organizing attitude.[19] Other attitudes about candidates, parties, and issues tend to be less ego-involved and hence are less central. I find it useful to extend the metaphor of centrality and think of party identification as a nucleus surrounded by clusters of satellite attitudes, with each cluster composed of attitudes about closely related objects.[20]

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[19] On centrality, see Theodore Newcomb, Ralph Turner, and Philip E. Converse, *Social Psychology* (New York: Holt, Rinehart and Winston, 1965), pp. 141–145; and Sperlich, *Conflict and Harmony*, chap. 6.

[20] The spatial metaphor rests upon the definition of "nucleus" as "a central point, group, or mass about which a gathering, concentration or accretion takes place" and "satellite" as "something attendant, subordinate or dependent." I want to avoid the concept

Each of the attitudes, as valenced cognitions, carries some positive or negative affect with respect to the immediate objects of the attitudes, and frequently toward the parties as well. The majority of the partisan valences on the satellite attitudes are of the same sign as the nuclear party identification attitude. There tend to be high correlations within clusters, for example, between attitudes about presidential and vice-presidential candidates of the same party, or between one's attitudes about school integration and job integration; but there tend to be low correlations between clusters: for example, one may think highly of a presidential candidate while disapproving of his position on civil rights. For the average voter, this cognitive structure is incomplete and inconsistent. Only those with a high level of political involvement are likely to have attitudes about a full range of political objects; only strong partisans are likely to have the same partisan valence on each of the attitudes.

Conceiving of a cognitive structure as a nucleus with satellites has some useful implications. So far as voting choice is concerned, it implies a difference between a first-order calculus, in which the vote is largely determined by party identification, and a second-order calculus used when party identification does not provide a guide (as with Independents who do not lean toward either party) or when the net effect of the satellite attitudes leads one to depart from his normal voting habits. This suggests some effects of issues and our consequent need to pay attention to them. We do not—on grounds of parsimony—need to know about them when their effect is simply to reinforce a first-order decision to vote in accord with one's party identification. We very much need to know about issues—and their relative significance within the total set of satellite attitudes—when a second-order decision is being made.

This conception also hints at a long-range effect of issues. Party identification is resistant to change. Many have commented on this, but Hugh Douglas Price has made the additional observation that it is a lagging indicator.[21] This suggests that the valence on the nuclear partisan attitude may be changed after most of the satellite attitudes come to have opposite valences. And in this respect, no one has better interpreted the effect of issues than Richard Boyd in a previous *Review* article:[22]

> In short, the impact of a candidate is substantial but of short duration. The impact of issues, while rarely great at any moment, accumulates over a period of time. Overall, issues may outweigh candidates in affecting the outcome of elections, for issues have the capacity to alter the greatest single determinant of a vote, party identification.

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of "system" in discussing the cognitive structure of the average voter. It now appears there is too much inconsistency between the clusters, and too little ability to predict how a change in one cluster will affect other clusters, to justify the use of the system concept.

[21] Hugh Douglas Price, "Micro- and Macro-politics: Notes on Research Strategy" in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge, Mass.: Harvard University Press, 1968), p. 121.

[22] Boyd, "Voting Defection," p. 510.

Rejoinder to "Comments" by Richard A. Brody and Benjamin I. Page and John H. Kessel

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Only two presidential elections ago, V. O. Key worried about the effect of theories of electoral behavior on practicing politicians. "If they see voters as most certainly responsive to nonsense," he warned, "they will give them nonsense. If they see voters as susceptible to delusion, they will delude them."[1] Perhaps in response to Key's concern, the contributors to this symposium and other writers have sought to discover, and frankly to emphasize, the importance of issues in voting. I believe we have accomplished this central purpose. It is no longer "perverse and unorthodox," as in Key's time, to argue that "voters are not fools," and that elections are meaningful democratic acts.

This basic conclusion should be marked before discussing remaining and detailed points of contention. After reading two articles, two commentaries, and two rejoinders, one is likely to be overwhelmed by problems of operationalizing the "normal vote," or defining "liberalism," or calculating the "partisan issue alignment index." Of course, these are important scholarly problems, and Brody and Page particularly have outlined the immediate needs in future investigations of the subject. Rather than explore the subject further at this point, however, I want to give the patient reader some relief, and will respond only to a limited number of the comments.

John Kessel has presented an able summary of the recent literature on issue voting, and of the concomitant problems in analysis. I believe, however, that he finds more emphasis on the subject in *The American Voter* than truly exists. That book's six-component model of voter choice is not an independent test of issue voting, as Kessel states. Rather, it is based upon the same open-ended questions used to demonstrate low ideological awareness among the electorate. Moreover, these six components are devalued as only short-term factors, "the attitudes immediately supporting voting choice."[2] In the Michigan group's overall analysis, voter attitudes are clearly subordinated to party identification. To quote from the gospel, "We are convinced that the relationships in our data reflect primarily the role of enduring partisan commitments in shaping attitudes toward political objects."[3]

Despite this emphatic conclusion, it is important to note that the relative importance of party identification and policy attitudes was not statistically tested in *The American Voter.* When Arthur Goldberg did perform such a test, using the same 1956 data, he found the Michigan model inadequate. Attitudes were seen to exercise an independent influence upon the vote.[4]

As to Professor Kessel's specific comments on my article, I need only respond to two points. First, I do not find the strictly statistical objections important. The absence of temporal variation from 1964 to 1968, when controls are introduced, only strengthens my argument that major political change occurred in the 1964 election and persisted in 1968. It is true that the partial correlations are lower than the zero-order relationships, but even the control generally reduces these figures by less than half.

More important is Kessel's substantive complaint that the questions employed (and my subsequent analysis) commingle two separate attitudes—the specific policy issues, and the general question of federal governmental power. He is certainly correct. Medical care or racial integration and the power of the national government are indeed joined in these survey questions. At more than one point in my article, I have noted that "liberal" and "conservative" denote attitudes toward the use of federal power for certain policy objectives.[5]

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[1] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1964), pp. 6ff.

[2] Donald E. Stokes, Angus Campbell, and Warren E. Miller, "Components of Electoral Decision," *American Political Science Review,* 52 (June, 1958), 368.

[3] Angus Campbell, Philip Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), p. 135.

[4] Arthur Goldberg, "Discerning a Causal Pattern among Data on Voting Behavior," *American Political Science Review,* 60 (December, 1966), 913–922. Independents are omitted from Goldberg's analysis, thereby increasing the relative importance of party identification.

[5] The point was made in the original draft of this article which I sent privately to Professor Kessel for his comments. In his kind reply. he noted the overlap of the two attitudes, and I subsequently tried further to clarify the operational use of "liberalism." In his comments here, he is probably refering to my earlier draft, which has been improved, I hope, by attention to his private critique.

But these equivalences are more than stylistic conveniences, for they are joined in the real political world, where only the federal government has shown the ability and occasional willingness to insure minimum standards of health care for all, or to open segregated schools to black children. The vanishing breed of liberal Republicans may have a more sophisticated philosophy, but segregationists and medical lobbyists know that their political interests are well-served by the championing of states' rights. Moreover, to the not insignificant extent that the mass public is aware of these terms, its explanation parallels my use of these terms.[6]

Brody and Page make only one specific criticism of my article: that I do not consider the possibility of partisan realignment. To the contrary, I raised this very possibility by hypothesizing that 1964 represented a "critical election," in which significant numbers of voters changed their party loyalties to conform with their policy preferences, and Table 1 in my article presents data on this point. Admittedly, I have not demonstrated realignment, but I have hardly neglected its likelihood; indeed, I am more likely to have exaggerated it.[7]

A final, more general point on issue voting. In many electoral analyses, issues are considered only as short-term influences, distinct in their effect from partisan loyalty. *The American Voter* and Brody and Page's comments are partially illustrative. This assumption is analytically convenient, but it underestimates the long-term effect of issues. One of the sources of party identification itself is policy attitude, and attitudes first evident in one election may continue to be relevant and influential in succeeding contests. The obvious example is the contemporary Democratic majority, which can be traced to policy attitudes formed during the New Deal. The point is inherent and treated well in Boyd's analysis. It should be remembered by all electoral researchers, lest they again treat American party loyalists as unthinking prisoners of tradition and habit.

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[6] See Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed., David E. Apter (London: The Free Press, 1964), pp. 219–223. A quick analysis of some other 1968 data indicated that over two-thirds of the sample could determine the relative conservatism or liberalism of the two major parties, and that 87 per cent agreed that the Democrats were the more liberal party (including 94 per cent of the strong Republicans). See deck 12, column 49 of the 1968 survey.

[7] See Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), pp. 119–122 and Donald J. Devine, *The Attentive Public* (Chicago: Rand McNally, 1970), pp. 95–99.

Rejoinder to "Comments" by Richard A. Brody and Benjamin I. Page and John H. Kessel

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The commentaries of Kessel and of Brody and Page raise fundamental questions of normal vote analysis. Kessel is concerned with the legitimacy of applying the technique to an election contested by three major candidates, given that we have normal votes only for a Democrat and a Republican. Brody and Page emphasize the problems of disentangling cause and effect in the relationships of issue positions and vote choices. Since a reply to Brody and Page would be moot if the objection of Kessel cannot be satisfied, I shall consider Kessel's commentary first.

Two Parties, Three Candidates

If we have normal vote estimates only for the Republican and Democratic parties, how should we treat the Wallace candidacy? My solution (defended in footnote 11) was to consider the Wallace vote a short-term deviation from the expected two-party vote and to keep the observed votes of the three candidates separate. An equally plausible assumption, Kessel argues, is that the normal Democratic vote was split between non-Southern and Southern Democratic candidates and that the expected Democratic vote should be compared with the *combined* vote of Wallace and Humphrey. While adding the Humphrey and Wallace votes might seem attractive (particularly to Hubert Humphrey), this solution has its own problems.

First, Kessel's treatment of Wallace as a Southern Democratic candidate is based on a premise that Wallace's votes came predominantly from Democrats and Independents. Although Independents did vote for Wallace in large numbers, a Wallace voter was no more likely to be a Democrat than was any randomly chosen number of the electorate (see Table 1). In fact, Wallace's party coalition no more resembled Humphrey's coalition than Nixon's.

Second, treating Wallace as a Democratic candidate not only distorts the candidates' *party* coalitions, it conceals important differences in their *issue* coalitions as well. Note, for example, the differences in the civil rights attitudes of those who voted for the three candidates. In all, 61 per cent of the electorate endorsed the view that civil rights leaders were pushing too fast. Eighty-seven per cent of the Wallace voters endorsed this view, as did 66 per cent of the Nixon voters; only 49 per cent of the Humphrey voters concurred. Adding the Humphrey and the Wallace vote obscures the fact that the racial attitudes of the Wallace voters were closer to Nixon voters than to Humphrey voters.[1]

Table 1. The Party Coalitions in 1968

	Democrats	Independents	Republicans		N
Nixon Voters	17	32	51	100%	490
Humphrey Voters	78	17	5	100%	421
Wallace Voters	46	41	13	100%	116
All Party Identifiers	46	30	24	100%	1531

Data Source: 1968 SRC Election Study

Finally, Kessel notes that if the Humphrey and Wallace votes are combined, the issue of the speed of civil rights progress almost disappears as an important issue in 1968. That conclusion in itself should make one chary of not distinguishing the Wallace and the Humphrey vote. Quite obviously, without the racial issue Wallace would not even have been a candidate in 1968. Thus, while I do not wish to minimize the problem of applying the normal vote technique to the 1968 election, I would argue that Kessel's particular method of reanalyzing the data is inappropriate.

Issue Voting or Rationalization

Brody and Page are concerned with another of the intractable problems of the analysis of issue voting. In their view the evidence that issues importantly influence people's votes can be explained away by what they term rationalization. Though they never define the term precisely, rationalization appears indistinguishable from what they later call projection and persuasion. Projection occurs when a voter falsely perceives his preferred candidate's position to be his own. Persuasion takes place if a voter adopts the issue position of his preferred candidate. In both cases vote choices are influencing issue positions, rather than issues determining votes. Conceiving of rationalization as a response set, Brody and Page construct a test to determine whether or not rationalization in fact contaminates the evidence for issue voting. While their test tends to refute the rationalization hypothesis in the 1968 data, they have left the status of V.O. Key's work in doubt. To discuss Key's evidence, it is important to distinguish the cognitive and the affective components of attitudes.

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[1] This is true of all of the issues included in Figure 5 of my original article.

Consider, for example, the following item from the Brody and Page commentary: "Which party can better handle the problem of keeping us out of a bigger war?" For most people, the question may be so heavily laden with affect that it has little cognitive content. It is like asking a person which of the parties he likes better in different words. Answering the question in such a way that the response correlates with one's vote requires neither interest in the issue nor any knowledge of the history of the two parties in committing the United States to war. A voter who had fixed on a voting choice would simply respond that this preferred party was better able to keep the United States out of war.

Contrast the above item with the following one, drawn from Key's analysis of the New Deal realignment: "Do you think the Wagner Act should be revised, repealed or left unchanged?"[2] The item gives no clues of the positions of the parties on this issue. If responses to this item are systematically related to the vote (i.e., if Republicans who favored the act voted Democratic and if Democrats who opposed it voted Republican), it is probably because the voters knew the differing Republican and Democratic positions on collective bargaining. An item such as this, then, has a substantial cognitive component. If an issue is salient enough that people become familiar with the parties' stands, then the issue position probably influenced their voting choices rather than vice versa.

In the 1968 data I presented, only one item is cast in such a way as to be vulnerable to a rationalization response set. It is the one which asks people, "In general, how do you feel about how President Johnson has done his job." The remainder of the items give away no clues of the positions of candidates or parties. But what of Key's data in *The Responsible Electorate*? In Chapter 3 Key outlines the class and issue bases of the emerging New Deal Democratic majority. In the main his evidence depends on items that had a clear cognitive content. This portion of his argument stands up well on reexamination. Unfortunately, this is not as true of the later chapters on elections of the 1950s and 1960s. Here, many more of his items are of a form in which affect toward the parties or the candidates is a central component, e.g., "Which presidential candidate—Stevenson or Eisenhower—do you think could handle the Korean situation best?"[3] Much of this evidence is suspect because of its possible contamination by rationalization.

The Normal Vote and Party Realignment

Brody and Page sound a second warning against normal-vote analysis. They point out that the technique considers issues "one by one in isolation from each other [so] . . . that the approach is continually reapportioning the same observed variation in the vote." Yet, in a sense this is one of its advantages. At a glance, one can see the degree to which voters who share a common position on a selected issue also support the same party and candidate. Differences in the expected party vote between groups with different opinions reveal the degree of ideological coherence of the parties on the issue at that point in time. If one then examines changes in the partisan components of an issue across a series of elections, one can show, as Pomper recently has, the pace at which the electorate has moved toward ideological clarity in its party attachments over time.[4]

In addition, the deviations of the observed vote from the expected vote point to the issues that appear important in changes in these party alignments. That is, in one election an issue may emerge which is associated with deviations from the expected party vote. By the next election enough people may have either changed their opinions or changed their party identification so that the issue cleavage is now manifest in a new party cleavage. This seems to have been the case with civil rights attitudes in the United States from 1956 to 1968.[5] Normal vote analysis can therefore clearly demarcate the issues that underlie party realignments.

Brody and Page recommend loss-function models as an alternative to the normal vote. I conclude by posing one question of the most recent variants of loss-function equations proposed by Brody and Page and by Shapiro.[6] These models, particularly Shapiro's, permit each voter substantial latitude in selecting the criteria on which he evaluates the candidates. They also allow each voter to make his own determination of the salience of these criteria and permit voters to misperceive the candidates' stands on issues. Undoubtedly, these equations should predict the votes of each individual with accuracy, but accuracy is not the only criterion for the evaluation of a theory. The cost of this increased accuracy of predicting individual votes may be that we cannot apply these theories to normative democratic theory or to analytical theories of party competition and electoral change. For example, what can we say about democratic control of policy through elections, if our theories of the individual vote decision do not presume substantial agreement on the criteria by which people evaluate candidates. Similarly, what interesting propositions about party competition and electoral change can we generate if we cannot assume that people know with reasonable accuracy the policy positions of the parties.[7] Unless the theories of the psychology of the vote decision are cast so that they can be integrated into broader theories of democratic control and party competition, then we may have solved less important problems at the expense of more critical ones.

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[2] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966), p. 47.

[3] Key, p. 78.

[4] Gerald M. Pomper, "Toward a More Responsible Two-Party System? What, Again?" *Journal of Politics*, 33 (Nov., 1971), pp. 916–40.

[5] Pomper, pp. 925–26.

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[6] Richard A. Brody and Benjamin I. Page, "The Assessment of Policy Voting: A Commentary," *American Political Science Review*, 66 (June, 1972), 450–458, and Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), 1106–19.

[7] This undoubtedly is why Davis, Hinich, and Ordeshook assumed that "all citizens make identical estimates" of candidates' stands on issues. Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review* 64 (June, 1970), 431.

A Theory of Neighborhood Problem Solving: Political Action *vs.* Residential Mobility*

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People who don't like the way things are going in their neighborhood have three broad alternatives. First, they can resign themselves to the situation and, doing nothing, just tolerate it. This is no doubt what most people do in response to the minor problems of city life—things that irritate them but don't seriously inconvenience them—since finding a solution is very often costly in time, energy and money.

When problems are sufficiently bad or longstanding, however, they can (second) leave the neighborhood altogether, or (third) they can stay and make some attempt to change things. This paper is about what people decide to do in response to neighborhood problems, why they decide to do it, and the consequences of their decisions for the neighborhood itself. It is based on a simple choice model that is elaborated using survey data about people facing neighborhood problems in different parts of the metropolitan area.

Albert Hirschman has developed a general theory of response to decline in organizations that is based on the latter two options.[1] He points out that "exit" and "voice" are the alternative ways an individual can respond if he wishes to maintain some level of performance from an organization whose performance is declining. Exit is simply quitting the organization or switching to a competing product. Voice is

> . . . any attempt at all to change, rather than to escape from, an objectionable state of affairs, whether through individual or collective petition to the management directly in charge, through appeal to a higher authority with the intention of forcing a change in management, or through various types of actions and protests, including those that are meant to mobilize public opinion.[2]

Since Hirschman's terminology fits the options we have in mind exactly, we have adopted it here. A resident exiting from a metropolitan neighborhood simply decides that some other location offers him more of what he wants, so he moves to take up residence in the new place. A voicing resident, in the more usual terminology of political analysis, is "making demands," "articulating his interests," or "participating in political action." As Hirshman says, voice is a more "messy" option than exit;[3] moving out of an area is one simple act, while voice can be individualistic, as in writing to newspapers, contacting public officials, signing petitions and voting, or it can be collective, as when people work through existing groups or form new ones.

Hirschman deals with cities only in passing, but many of his ideas are directly applicable to them. His main concern is with the consequences for the organization of the particular response its "customers" make to decline in performance; will it tend to further the decline, or will it tend to bring the organization back to better performance levels? This is our main concern with cities, too. Is neighborhood exit or neighborhood voice more likely to improve things that are going bad in a neighborhood? If one works better than the other, then it be-

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[1] Albert O. Hirschman, *Exit, Voice, and Loyalty: Response to Decline in Firms, Organizations, and States* (Cambridge: Harvard University Press, 1970).

[2] Hirschman, p. 30. Notice that the economic perspective adopted by Hirschman is paralleled by a social psychological perspective. "Avoidance" is essentially exiting behavior; "confrontation/attack" is essentially voice; and "tolerance" is equivalent to our term "resignation"—the situation when people experience problems but do nothing about them.

[3] Hirschman, p. 16.

comes important to know about the conditions under which people are likely to do either.

Our general thesis is that voice, not exit, is the mechanism most likely to bring about improvement in metropolitan neighborhoods. This principle is not something we can demonstrate from our data, but it does have *prima facie* plausibility. When neighborhood problems arise from *density*, then mobility out of the area is likely to help things somewhat. But in most cases, exit will deplete a neighborhood of exactly those resources that are required to effect an improvement. Money is one of those resources; people who have it often use it to make the move to a more satisfactory neighborhood, thereby removing part of the area's tax base and, in general, weakening the area's ability to effect change.

Energy and concern are other resources lost to neighborhoods by exit. Hirshman makes this point in discussing differences among people in concern about quality and about price. Quality-conscious people, he argues, will be the first to exit when quality declines and—compared to price-conscious people—will be willing to pay higher costs for a different product. He illustrates this proposition in the case of neighborhoods: those who are most concerned about quality in schools, services, and neighborhood conditions will be the first to migrate to areas where those things are better, assuming, of course, that such areas exist. But these are the people most likely to get involved in community action if they *do* decide to remain. After they are gone, they "will be lost to the citizen's groups and community action programs that would attempt to stem and reverse the tide of deterioration."[4]

On the other hand, we cannot make the easy assumption that neighborhood problems will be solved *to the extent* that residents make demands on their local governments for change. Governments vary greatly in their ability to take effective action, assuming that they are motivated to do so.[5] Given the ability, they may not have the motivation. Michael Parenti cites case studies of attempts by ghetto blacks to get action on slum rentals and a needed traffic light to illustrate how well-organized collective action can have no consequences at all.[6] Crain and Rosenthal have argued that high levels of community participation can actually *decrease* the prospects for action on something the majority favors since the opposing minority is also likely to become mobilized and can often block action.[7]

Similarly, neighborhood problems are sometimes eliminated without any demands on the part of local residents; voice is neither a sufficient *nor* a necessary condition for change. "Spillover" from action taken by one local administration can improve things in another's territory,[8] or changes can come about through the undirected working of economic forces. A "public-regarding" city elite might be expected to work for improvement without prodding from the residents of a neighborhood.[9] Similarly, voice by people in one area can help people in another area who do nothing or exit. For example, if the residents of one suburb are affected by air pollution and are successful in attempts to curb it at the source, people in all areas benefit.

On the other hand, if the same suburban residents manage to increase the number of police patrols in their neighborhood, this will have little effect on crime in other areas—indeed, it might *increase* crime elsewhere by reducing police protection in order to meet the demand in the first area. The matter is complicated still further because demands are made on a number of different governments within the metropolitan area. Nevertheless, demands can make an important difference. This view has been a basic assumption of liberal-democratic theory for years, and it will be our assumption here. *How much* the chances of action are increased by demand making depends on factors such as those just discussed.

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[4] Hirschman, p. 51.

[5] We are not talking here about symbolic actions that are often a substitute for real change. This propensity of governments to respond symbolically to demands for change has been discussed with considerable insight by Michael Lipsky, "Protest as a Political Resource," *American Political Science Review*, 62 (December, 1968). 1144–1158 and Murray Edelman, *The Symbolic Uses of Politics* (Chicago: University of Illinois Press, 1964).

[6] Michael Parenti, "Power and Pluralism: A View from the Bottom," *Journal of Politics*, 32 (August, 1970), 582.

[7] Robert L. Crain and Donald B. Rosenthal, "Community Status as a Dimension of Local Decision-making," *American Sociological Review*, 32 (December, 1967), 970–984.

[8] See Alan Williams, "The Optimal Provision of Public Goods in a System of Local Government," *Journal of Political Economy*, 74 (October, 1956), 18–33 for a discussion of "spillover effects" among local governments.

[9] For a discussion of the concept "public-regarding elite" see James Q. Wilson and Edward Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 58, December, 1964), 876–887. An important critique of the concept and analysis based on it is contained in Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *American Political Science Review*, 60 (June, 1966), 306–326. We should remember that some kinds of neighborhood voice—using the term in the broad sense adopted by Hirschman—do not require action of any kind by city elites. Neighborhood beautification groups, for example, can make the required improvements quite independently of community power structures. But, in general, problems that can be solved in this way will be relatively minor.

Assumptions about Choice

The importance of Hirschman's work derives, in part, from his recognition that voice and exit are *alternatives* for a "customer" faced with "decline" in a "product." A large body of literature deals with the reasons why people leave a neighborhood, and another deals with participation in urban politics; but in neither of these—to our knowledge—is it recognized that these are alternatives, that a person might take political action *instead of* leaving, or vice versa. Studies of mobility usually see the only alternative to a move as accepting the present state of affairs in the current neighborhood, and studies of participation usually see the only alternative to action as apathy or resignation.[10]

Our data will document the propensity of people to exit or to voice when they are faced with a neighborhood problem. Unfortunately, we do not have direct data about why people in actual decision-making situations do one thing and not another. We *assume* that a person decides whether to exit, to voice, or to do nothing by weighing the costs of a particular action against the benefits likely to accrue from it. As a rational decision maker, he does that which offers him the greatest benefit for the least cost. For example, suppose someone living in a central district dislikes conditions in his neighborhood. For one reason or another, he decides that an attempt to improve things there would be very costly (in time, energy, and money) and, in the long run, would make little difference. While a move to the suburbs would involve an expensive house and perhaps higher taxes, the difference in conditions there is vast. Rationally, he makes the move.

If these considerations were all the decision involved it is unlikely voice would ever be used much; exit is just too simple and "clean" a solution to a bad neighborhood. But in fact certain constraints limit the ability of people to use the exit option. Money is one such constraint. People with high incomes can choose from the wide variety of residential areas available in the metropolitan area, while people with low incomes have far fewer options. We usually think of high income people "escaping" the problems of the city—as often as not the "problems" are invading blacks—by a retreat to the suburbs, but if they wish they can also escape the problems of their suburb by a move to a different suburb, or perhaps to a city apartment. On the other hand, people with low incomes are confined to the options available in low-income areas (excluding high-rise luxury apartments), and a move is far less likely to involve a change in neighborhood conditions. Race also constrains the ability to exit. Substantial differences in neighborhood type within black ghettoes have been documented,[11] but blacks are limited to the options offered by the ghetto.

The viability of the voice option differs among types of people and among types of neighborhoods. Pluralists and antipluralists differ about the extent to which political resources are distributed in the population,[12] but they generally agree that income and education confer substantial advantages. Lipsky has qualified this tenet somewhat by pointing out that "relatively powerless groups" can sometimes get much of what they want through protest; the skillful use of protest can mobilize other groups which have political resources to help a group that does not.[13] But as Lipsky points out, a successful protest requires formidable organizational and leadership resources, quite apart from some support in the general population. The political weakness of poor people is well summed up by Parenti:

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[10] Hirschman points out that both economists and political scientists have a "trained incapacity" to recognize the importance of one of the two mechanisms. Economists, he says, have a bias in favor of exit and against voice: "The economist tends naturally to think that his mechanism is far more efficient and is in fact the only one to be taken seriously" (p. 16). On the other hand,

> Political scientists have long dealt systematically with [the function of interest articulation] and its various manifestations. But in doing so they have ordinarily confined their attention to situations in which the only alternative to articulation is acquiescence or indifference (rather than exit), while economists have refused to consider that the discontented consumer might be anything but dumbly faithful or outright traitorous (to the firm he used to do business with (pp. 30–31).

[11] See, for example, Wilfred G. Marston, "Socioeconomic Differentiation Within Negro Areas of American Cities," *Social Forces*, 48 (December, 1969), 165–176.

[12] Classic statements of the pluralist position are found in: Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961); Arnold Rose, *The Power Structure* (New York: Oxford University Press, 1967); Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); Edward Banfield, *Political Influence* (New York: Free Press, 1961). In addition to the earlier work of C. W. Mills and Floyd Hunter, antipluralist arguments are found in: Peter Bachrach and Morton Baratz, "Two Faces of Power," *American Political Science Review*, 56 (December, 1962); 950; Grant McConnell, *Private Power and American Democracy* (New York: Alfred A. Knopf, 1966); Theodore Lowi, "The Public Philosophy: Interest-Group Liberalism," *American Political Science Review*, 61 (March, 1967), 5–24.

[13] Lipsky, "Protest as a Political Resource," pp. 144–158.

> Party politicians are inclined to respond positively not to group *needs* but to group *demands,* and in political life as in economic life, *needs* do not become *marketable demands* until they are backed by "buying power" or "exchange power" for only then is it in the "producer's" interest to respond. The problem with most lower-strata groups is that they have few political resources of their own to exchange.[14]

But some problems are beyond the resources governments have available, no matter what political resources or "purchasing power" a group manages to mobilize. Most obviously, this is the case for city governments in areas drained of resources by the movement of wealth to the suburbs; problems in such areas are normally far worse than those in the suburbs, and resources for dealing with them far smaller. Even with the greatest governmental good-will, voice may still be an ineffective strategy in such cases.[15]

Stated in these terms, the model implies that poor people are at a double disadvantage: they are handicapped in their ability to use both exit and voice for solving their neighborhood problems. In such cases passivity can be rational since a dispassionate assessment of the other alternatives could suggest that action of any kind would be a waste of time, energy, and money. And, of course, the situation is worse for blacks, whose ability to use exit or voice is also constrained by their race. Anthony Downs makes the point that political action (voting) can be irrational if policy changes have no net effect on individual welfare;[16] we can add that exit, too, can be irrational if *it* makes no difference.

The Social Sciences on Exit and Voice in Cities

The literature on urban mobility has paid the greatest attention to movement from urban areas to the suburbs; the dominant thesis is that migrants in this direction are mainly people who find urban life incompatible with their "family-oriented" life-style. Wendell Bell, for example, has shown that the main reason given for a move to the suburbs is that "it would be better for the children there"—more space, bigger houses, better schools, "nice" children to play with and so on.[17] The conclusion is supported by a great deal of other research.[18] The suburban move is best seen as one outcome of an interaction between the demands an individual places on his metropolitan neighborhood and what that neighborhood—and "competing" ones elsewhere—can offer.

Despite this emphasis on movement toward the suburbs (and other literature dealing with the "rootlessness" of suburban populations), urban areas appear to have higher rates of mobility than suburban ones, and most of the movement is *between* urban neighborhoods rather than *toward* the suburbs.[19] *General* theories of metropolitan mobility differentiate between "push" and "pull" factors—things having to do, respectively, with negative characteristics of the present area and positive characteristics of the new one.[20] In these terms, Lansing and Barth conclude that dislike of the present neighborhood contributes most to a family's intention to move, followed by dissatisfaction with its present housing, overcrowding, and a desire to move either farther out or farther in.[21] Sabagh and his colleagues conclude: "Needs and aspirations pertaining to the dwelling and the neighborhood are one of the most important determinants of residential mobility."[22] "Problems in the neighborhood" do seem to be the prime "pushing" factor behind exit from metropolitan neighborhoods; more attractive neighborhoods exercise "pull" relative to those problems.[23]

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[14] Parenti, "Power and Pluralism." Italics in the original.

[15] Cf. Edward C. Banfield's assertion in *The Unheavenly City* (Boston: Little Brown, 1970): ". . . the 'revenue crisis' mainly reflects the fact that people hate to pay taxes and that they think that by crying poverty they can shift some of the bill to someone else" (p. 9). Crying poverty is, at least, more reasonable for most city governments than for most suburban governments.

[16] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), p. 43.

[17] Wendell Bell, "Familism and Suburbanization; One Test of the Social Choice Hypothesis," *Rural Sociology*, 21 (September-December, 1956), 276–283.

[18] For other literature dealing with movement toward the suburbs and familism, see: Roy H. Rodgers, "Some Factors Associated with Homogeneous Role Patterns in Family Life Cycle Careers," *Pacific Sociological Review*, 7 (Spring, 1964), 38–48; Sylvia Fleis Fava, "Suburbanism as a Way of Life," *American Sociological Review*, 21 (February, 1956), 34–37.

[19] See John B. Lansing and Nancy Barth, *Residential Location and Urban Mobility: A Multivariate Analysis* (Report prepared for the U.S. Department of Commerce, Bureau of Public Roads, by the Institute for Social Research, University of Michigan, 1964). Richard Sennett also points out this greater "exit-proneness" of urban residents in *The Uses of Disorder*, (New York: Alfred Knopf, 1970), pp. 152–153, in his discussion of loose social controls in such areas.

[20] For a useful summary of the findings in these terms, see George Sabagh, Maurice D. Van Arsdol, Jr., and Edgar W. Butler, "Some Determinants of Intrametropolitan Residential Mobility: Conceptual Considerations," *Social Forces*, 48 (September, 1969), 88–98. Charles M. Tiebout's "The Pure Theory of Local Expenditure," *Journal of Political Economy*, 64 (October, 1956), 416–424, is useful from an economic perspective.

[21] Lansing and Barth, p. 14.

One reason that exit has not been seen as an alternative to voice may be that the "political participation" literature characteristically fails to treat "participation" as a problem-solving act. Of course, there is good evidence that the act of voting is often better explained by the norm of civic duty than by any expectation that particular situations will be changed by it.[24] But people participate in politics in ways other than voting, and ample evidence suggests that there are strong norms *against* some of them, such as protest activity and demonstrations.[25] These more "difficult" behaviors seem more likely to be genuine problem-solving acts, and they are what we are chiefly interested in. By failing to emphasize this problem-solving character of much political participation, researchers have failed to consider that other kinds of actions—nonpolitical actions—might be a far more efficient way of doing things.

Although perhaps broad theoretical works do imply a problem-solving motive when they talk about "demand making," empirical studies of participation characteristically focus on status and psychological-attitudinal "determinants" of voting. High-status people are found to participate more than low-status people, men more than women, the middle-aged more than the young and the old—and so on.[26] Alienation is often seen as a prior cause of low participation,[27] and the same role is seen for apathy. People fail to take part in political action because they are apathetic, or perhaps because they have a low sense of their political efficacy. *The American Voter,* for example, argues for a causal sequence from education "through ego strength to political efficacy and on to increased political participation."[28]

The literature about black participation contains some anomalies that might, in part, be explained by changing historical circumstances. Although there are some exceptions, most research conducted in the 'fifties and early 'sixties found that blacks participate less in politics than whites do.[29] Later research, however, has shown that when socioeconomic status is controlled, blacks actually participate more than whites. Marvin Olsen found that this greater black participation occurred in a wide variety of social activities as well as politics, and also that it increased between 1957 and 1968.[30] The slowly changing racial climate of the 'sixties, coupled with increasing black dominance of the central areas, is probably changing traditional black political passivity.

From the perspective of our model, the usual explanations of political participation tell only part of the story. By *starting* with variables that are "inside the individual's head"[31] they do not give due recognition to the social and political *context* of participation and how his perception of that context influences what the individual does. That is the crux of our model: An individual's participation is seen as a response to a need he experiences in his environment (or at least the neighborhood part of it), and as directed toward some agency, usually government, that has the power to do something about it. We suspect that attitudes fit most satisfactorily into an explanatory scheme as *consequences* of the way an individual assesses his social and political environment: that these assessments influence *both* attitudes *and* behavior, and that they are prior to both—although reciprocal relationships undoubtedly exist[32]

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[22] Sabagh et al., p. 95.

[23] The literature suggests that "problems" that push people into mobility are, by comparison, seldom unique to a particular family in a neighborhood. For example, having to commute a long way to work is infrequently given as a reason for movement and the same goes for having friends in another part of the city. See: Peter Rossi, *Why Families Move,* (New York: Free Press, 1955), 90–92; Sabagh et al., "Some Determinants of Intrametropolitan Residential Mobility."

[24] See especially, Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1960) for a discussion of civic duty and similar variables related to voter participation.

[25] See: John P. Robinson, "Public Reaction to Political Protest: Chicago, 1968," *Public Opinion Quarterly,* 34 (Spring, 1970), 1–9; Philip E. Converse et al., "Continuity and Change in American Politics; Parties and Issues in the 1968 Election." *American Political Science Review,* 63 (December, 1969), 1083–1105; Marvin Olsen, "Perceived Legitimacy of Social Protest Action," Social Problems (Winter, 1968), 297–310; Ralph Turner, "The Public Perception of Protest," *American Sociological Review* (December, 1969), 815–830.

[26] For a summary of findings regarding political participation, see Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1963). See also Angus Campbell et al., *The American Voter;* Gabriel Almond and Sidney Verba, The *Civil Culture* (Princeton: Princeton University Press, 1963).

[27] For an excellent summary of the alienated voter hypothesis and a critique, see Robert L. Crain, Elihu Katz, and Donald B. Rosenthal, *The Politics of Community Conflict* (Indianapolis: Bobbs Merrill, 1969).

[28] Campbell et al., p. 518.

[29] Campbell et al., p. 316. Morris Janowitz and Dwaine Marvick, *Competitive Pressures and Democratic Consent,* (Ann Arbor: University of Michigan Press, Michigan Governmental Studies, No. 32), p. 26.

[30] Marvin E. Olsen, "Social and Political Participation of Blacks," *American Sociological Review,* 35 (August, 1970), 682–697; See also Nicholas Babchuck and Ralph V. Thompson, "Voluntary Associations of Negroes," *American Sociological Review,* 27 (October, 1962), 647–655.

[31] The phrase belongs to James S. Coleman, "Relational Analysis: The Study of Social Organization with Survey Methods," *Human Organization,* 17 (Winter, 1958–1959), 28–36. Coleman suggests that this shortcoming might reflect the heavy reliance these studies usually place on survey methodology. He points out the tendency of studies based on survey research to theorize about things that can be readily measured by this technique (that is to say, characteristics of individuals rather than of their environments); Coleman also systematizes the various modifications of

The behavioral literature on participation tells us little about how people assess different social and political environments. More generally, we get only sketchy impressions from the literature about the aggregate consequences of the multitude of individual exiting and voicing acts. The mobility literature does suggest that exit decisions result in a patchwork of more or less homogeneous neighborhoods throughout the metropolitan area: people obviously tend to seek out neighbors who are similar to themselves in status, race, and lifestyle. As the earlier discussion suggested, it is a far more complex process to link individual demand making to the outcomes of political systems and change in neighborhood conditions.[33]

Subsequent sections of this paper ask: What neighborhood conditions produce exit, and what conditions produce voice? What will keep voice-oriented people in a declining neighborhood? Are there any differences between racial and status groups in their propensity to use one means of coping or the other? Are there any differences between neighborhoods in this respect? Do people who relate differently to their neighborhood respond differently when faced with problems there? Micro-macro theorizing is, in general, underdeveloped in the social sciences, but several authors have pointed out its centrality to sociology and political science.[34] We cannot demonstrate the effect of different responses on neighborhood conditions themselves, but the concluding section draws on the data and the simple assumptions about aggregation that we outlined earlier to develop a model of problem solving in different metropolitan neighborhoods.

Data Collection and Coding

We used two types of data in this study—survey data to measure individual responses to problems, and aggregate data (from census tracts) to measure neighborhood differences. The survey was conducted in Columbus, Ohio in the spring of 1966.[35] In order to determine

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survey technology that take account of the individual's social context.

[32] Thus, if we think about alienation as a variable "inside the individual's head," at least two alternative models are possible: (1) a developmental sequence from assessments of the situation (the calculus) through attitude (alienation) to behavior (participation); (2) a spurious relationship in which the calculus influences alienation *and* participation, but alienation *in itself* does not influence participation. See Herbert Hyman, *Survey Design and Analysis* (New York: Free Press, 1955) pp. 254–263 for distinctions between these two types of process. See also Hayward Alker, *Mathematics and Politics* (New York: Macmillan, 1965), pp. 122–125.

[33] One important study suggests that decisions made by various metropolitan governments tend to maintain the social homogeneity of metropolitan areas. The argument implies that mobility and governmental action tend to work in the same direction of system maintenance. See Oliver P. Williams et al., *Suburban Differences and Metropolitan Policies* (Philadelphia: University of Pennsylvania Press, 1965).

[34] See, for example: Hubert M. Blalock, *Theory Construction* (New York: Prentice-Hall, 1969). Blalock points out that one might have to use weighted averages rather than arithmetic means in order to allow for the fact that, in collective decision making, some individuals count more than others (p. 6). James S. Coleman in *Introduction to Mathematical Sociology* (New York: Free Press, 1964) writes:

> . . . it is precisely *because* there is some varying structure of relations between individuals that sociology has a reason for existence. Sociology would otherwise be nothing more than an "aggregate psychology," as thermodynamics is an "aggregate statistical mechanics," and would be concerned only with distributions of motives, needs, beliefs, and so on. Many sociologists today, in fact, forget that sociology is different from aggregate psychology and base their "sociological" explanations on such distributions" (pp. 88–89).

One author who does make a formal attempt to spell out such linkages is Allen Barton, *Communities in Disaster* (New York: Doubleday, 1969), especially Chapter 5, "The Altruistic Community." See also: Heinz Eulau, *Micro-Macro Political Analysis,* (Chicago: Aldine, 1969); and Donald E. Stokes, "Analytic Reduction in the Study of Institutions," a paper delivered to the 1966 Annual Meeting of the American Political Science Association in New York.

[35] The best available sample frame was the *Columbus City Directory,* which proved to be a reasonably reliable listing of households in the metropolitan area; our survey is representative of the entire metropolitan area. Households were sampled from this frame using a table of random numbers, and individuals were sampled from households using the respondent selection key detailed in Charles H. Backstrom and Gerald D. Hursh, *Survey Research* (Chicago: Northwestern University Press, 1963), pp. 50–59. Interviews were conducted by graduate students and advanced undergraduates in political science. Extensive telephone checking revealed complete reliability of the interviewers. Eighty-three per cent of the interviews attempted were completed.

whether people were experiencing problems in their present neighborhood, we asked: "Are there any problems facing this area of Columbus?" We coded a variety of types of problems in addition to the simple yes/no dichotomy. The question used to investigate voice was drawn from the set used by Almond and Verba to measure civic competence:[36] "Suppose a regulation were being considered by the Columbus City Government that you considered unjust or harmful. What do you think you could do about it?" The answers were coded into such categories as: work through informal groups; work through a political party; vote; work through formal groups (churches, unions etc.); write letters; talk to powerful people; consult a lawyer, and so on. The answers were also dichotomized: all those who indicated some kind of action were coded in a "voice" category, and those who said they would do nothing or who could not mention anything were coded "no voice." We have already pointed out that our questions do not relate to particular actions directed to particular problems—data that would have been ideal. Responses to this question do indicate *proneness* to take action, however, and that is what we are interested in.

To measure "exit-proness" another straightforward question was used: "Do you have any intention of moving in the next year or two?" Notice that the intention to move is not necessarily the same thing as actually making a move; it is possible that many such intentions were never carried out. But data from Rossi's study of urban mobility suggest that, in fact, a high proportion of people who indicate such intentions do act on them.[37] Once again the questions do not tie the individual's action to the particular problems he mentions in the area (as the model does), but, once again, *proneness* to act tells us much of what we want to know.

The survey data were combined with census data in order to locate people in particular metropolitan contexts. A factor analysis across the 150 census tracts in Columbus identified a variable loaded on characteristics usually associated with the "urban-suburban" dimension, and each individual in the sample was assigned a factor score on this variable for the tract in which he lived.[38] This lets us define the individual's social context with some precision and also permits us to make some inferences about the political environment. We have trichotomized urbanism into "urban," "mixed" and "suburban" areas by cutting the range at plus or minus one-half standard deviation from the mean.[39] We should point out that these three "types" of neighborhoods are not *necessarily* contiguous areas. Mapping these factor scores shows, however, that the "urban" neighborhoods are, in fact, in the central part of the metropolitan area, the "suburban" ones are scattered around the periphery, and the "mixed" ones are—generally—between these two. We are measuring neighborhood *type*, not distance from the central business district, but the two are correlated. Notice also that "neighborhood" is defined operationally for each indi-

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[36] Almond and Verba, *The Civil Culture.*

[37] Peter Rossi, *Why Families Move.* We used the stronger question "intention to move" in preference to the weaker one "desire to move."

[38] A standard factor analysis was performed. The loadings on the unrotated urbanism factor were as follows:

	1960	*1950*
% population Negro	.5695	.4992
% employees white collar	−.6710	−.5786
% homes owner occupied	−.7882	−.8300
Median years of schooling	−.6885	−.5969
Median income	−.8410	−.8042
% houses built since 1950	−.6639	−.6406
Female employees per 1000 male employees	.5832	.5665
% population married	−.7397	−.7260
Sex ratio (females per 100 males)	−.2294	−.3427

As will be noted, there is a strong correlation (r = .92) between the loadings based on the 1960 census and the 1950 census. The 1950 census was used to define the urbanism value of the area a respondent had *last* lived in "for any length of time" when he moved before Jan. 1st, 1956; the 1960 census was always used to define the urbanism value of the present place of residence.

This variable should not be confused with the Shevky and Bell urbanization or family status variable, although it is undoubtedly correlated with it. That measure is loaded on familism and life-style variables while ours, in addition, has status and ethnicity components. See: Eshref Shevky and Wendell Bell, *Social Area Analysis* (Stanford: Stanford University Press, 1955).

[39] Notice that this combination of survey and census data permits "contextual analysis" relating environmental variables to the individual's own responses. See James S. Coleman, "Relational Analysis." For other examples of contextual analysis, see: Peter M. Blau, "Structural Effects," *American Sociological Review,* 25 (1960), 178–193; James A. Davis, Joe L. Spaeth, and Carolyn Husen. "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review,* 26 (1961), 220; Ernest Q. Campbell and C. Norman Alexander, "Structural Effects and Interpersonal Relationships," *American Journal of Sociology,* 71 (1965), 284–289; Arnold S. Tannenbaum and Jerald G. Bachman, "Structural Versus Individual Effects," *American Journal of Sociology,* 69 (May, 1964), 585–595; Warren E. Miller, "One-Party Politics and the Voter," *American Political Science Review,* 50 (September, 1956), 707–725; Robert D. Putnam, "Political Attitudes and the Local Community," *American Political Science Review,* 60 (September, 1966), 640–654.

vidual in the study as the census tract in which he lives.

Parts of the analysis to follow require that people be located in neighborhoods they have lived in previously. Respondents were asked the exact address at which they had last lived "for any length of time," and the urbanism score for the appropriate census tract was added to their survey responses. The problem of measurement error, of course, comes up in both of the urbanism indices, but it is most acute for past urbanism. The interviews were conducted in the winter and spring of 1966, and the census data on which the "present" values were based were drawn from the 1960 census. The time lag no doubt meant that the structure of some areas was different when the respondents were interviewed from what it had been when the census was taken six years earlier. Little can be done but admit this possibility. For the "past" scores, the possibility was that the move took place some time *before* the data were collected. To cope with this problem, we adopted the rule that if the respondent said he had moved before January 1st, 1956, his past urbanism score was coded from the 1950 census; if he moved after that date the 1960 census was used. The factor analysis of the 1950 data revealed essentially the same factor as did the analysis of the 1960 data, and the same coding procedures were used to define "urban," "mixed," and "suburban" areas.

Findings

Since the fact of residential segregation makes important differences among the options available to whites and blacks when faced with neighborhood problems, we have analyzed the choice patterns of the two races separately. We have used education as an indicator of status, dichotomizing for simplicity so that those with a high school diploma or more are considered "higher-status" and those with less than that, "lower-status."

Our first major findings for the white population are:

A. Whites:

1. *Higher-status whites are prone both to voice and exit in the face of neighborhood problems, but somewhat more to voice.*
2. *Lower-status whites are more prone to exit than to voice in the face of neighborhood problems.*

The data are given in Table 1. We have correlated the dichotomous "problem perception" variable with two other dichotomous variables—exit-proneness and voice-proneness. For high-status people, voice-proneness and exit-proneness are both associated with perception of problems, but for low-status people only exit-proneness is associated with problem seeing. Our model suggested that high-status people have reason to expect both voice and exit to be reasonably efficient mechanisms, and the data support the argument that, in fact, they do attempt both when faced with problems. But the opposite was also suggested for low-status people: neither exit nor voice seemed like very efficient mechanisms for them. In order to explain why low-status people so decisively reject voice in favor of exit, we need to know more about the situation they are responding to. Individual status might not be as important as the type of neighborhood people live in or as the objective basis for expecting that exit might work and voice might not.

Table 1. Exit and Voice among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Intending to Move and 'Political' Action Taking

	Problems and Exit	Problems and Voice
High Educated* (n=328)	.263 ($p<.05$)[a]	.321 ($p<.05$)
Low Educated (n=115)	.333 ($p<.13$)	.134 ($p<.30$)

* Education is dichotomized so that 'high' is high school diploma and 'low' is less than that.

[a] In this and subsequent tables Yule's Q is used to represent an association between two dichotomous nominal variables. The one-tail test was used to determine the significance after the Q value was transformed into a normal distribution Z score.

Tables 2 and 3 show the relation between experiencing problems in a neighborhood and, respectively, exit and voice when *both* education and urbanism are held constant. In other words, they show the propensity of people of different status to respond in different ways when they live in different areas. Response does depend, in part, on where a person lives. Specifically, for urban areas:

3. *The tendency for low-status whites to exit rather than voice is greatest in urban areas;*
4. *High-status whites are more prone to exit in urban areas than elsewhere, although they are also prone to voice in such areas.*

The data make it quite clear that high-status *and* low-status whites living in the urban areas reject the "political" solution in favor of the "non-political" one. Yet it is not that they do not *want* government to do anything; we asked

Table 2. Exit among Whites in Different Areas; Correlation (Yule's Q) Between Seeing Problems and Intending to Move

	Suburban Areas	Mixed Areas	Urban Areas
High educated	.184 (160) $(p<.22)$	−.081 (114) $(p<.39)$	.831 (51) $(p<.0001)$
Low educated	.316 (36) $(p<.27)$	.351 (45) $(p<.24)$	.287 (33) $(p<.29)$

respondents to name things they thought the city government should start doing—things it was not doing now—and the gamma between number of items mentioned and seeing problems in the neighborhood was .571 for the less educated population and .236 for the more educated one. These citizens have problems in their environment that might be dealt with by government, they believe government should be doing more than it presently is, and yet they withdraw from taking various political actions that, according to democratic theory, might be expected to get corrective actions out of government. Instead, they are prepared to move out of the district and try to find a better place to live. "Politically alienated" is a reasonable label to attach to such people; their alienation from government could be fully rational under some circumstances.

Hirschman's general theory of exit and voice suggests several possible reasons for this political alienation. First, sheer *number* of problems facing the resident of urban areas might make voice seem an unreasonably costly strategy in comparison to exit:

> As voice tends to be costly in comparison to exit, the consumer will become less able to afford voice as the number of goods and services over which he spreads his purchases increases—the cost of devoting even a modicum of time to correcting the faults of any one of the entities he is involved with is likely to exceed his estimate of the expected benefits for a large number of them.[40]

Table 3. Voice among Whites in Different Areas; Correlation (Yule's Q) Between Seeing Problems and "Political" Action Taking

	Suburban Areas	Mixed Areas	Urban Areas
High educated	.146 (159) $(p<.464)$	.432 (112) $(p<.06)$	.476 (50) $(p<.16)$
Low educated	.247 (36) $(p<.30)$	.286 (44) $(p<.25)$	−.256 (32) $(p<.29)$

We will return to the number and kind of problems people experience in different areas later; the point here is that a large outlay for voice in central areas *might not* go very far toward changing things in the direction wanted.

Second, we must take into account differing expectations that centers of power and decision making will respond to voice at all. In Hirschman's words: ". . . the decision whether to exit will often be taken *in the light of the prospects for the effective use of voice.* If customers are sufficiently convinced that voice will be effective, then they may well *postpone exit.*"[41] Parenti has shown that there is often good reason for the residents of central areas to be cynical about the prospects of voice bringing about the changes people want;[42] we can reasonably argue that the comparative reluctance of both status groups to use voice in the central areas is a reflection of the same assessment. Whether because the problems are seen as too big for government, or because government is seen as unresponsive to voice, people simply do not believe that government will help them very much. In light of this argument, it is interesting to notice that low-status voice *outside* the urban areas is comparable to high-status voice; in the mixed areas low-status people are only slightly less willing to use voice, and in the suburban areas they are somewhat *more* willing. We have already mentioned the standard finding that low-status people participate less in politics than high-status people do. The present data suggest that this finding might be a consequence of the fact that low-status people live disproportionately in urban areas where political action taking is simply less rational than elsewhere. When the *situation* is different, low-status people appear to take such action as frequently as high-status people.

While voice is rejected by these urban residents, exit is an attractive alternative. It is usually far less costly to try to find a better neighborhood than to try to change an impossible situation in the present neighborhood through problematic political mechanisms. The decision to move is more easily made by high-status residents who have the financial resources to pick

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[40] Hirschman, *Exit, Voice and Loyalty,* p. 40.

[41] Hirschman, p. 37. Italics in the original.

[42] Parenti, "Power and Pluralism." See also. Stanley Milgram, "The Experience of Living in Cities," *Science,* 167 (13 March, 1970).

Table 4. Relation between Exit and Voice among Whites; Correlation (Yule's Q) Between Intending to Move and "Political" Action Taking

	Suburban Areas	Mixed Areas	Urban Areas
High educated	−.504	.534	.533
	(85)	(58)	(34)
	(p<.09)	(p<.15)	(p<.20)
Low educated	−.379	−.846	−.091
	(18)	(14)	(15)
	(p<.30)	(p<.01)	(p<.45)

and choose a new neighborhood—as our model points out, the chances of successful exit are greater for high-status people. But exit is at least a *possible* alternative for low-status whites as well, and the fact that they are less likely to find a new neighborhood entirely to their satisfaction only dampens their willingness to try and does not eliminate it. The response of blacks—whose mobility options are constrained by segregation—to this inefficiency of the voice option will be dealt with later.

The data presented so far speak simply to the association between each of the response strategies taken separately and the perception of problems in the neighborhood. They do not tell us anything about the relation between exit and voice among different kinds of individuals, or about the frequency of the two in different kinds of neighborhoods. Neither do they tell us anything about the incidence of *resignation*—the rejection of both voice and exit as viable problem-solving devices. Table 4 includes only those respondents who reported problems in their neighborhood and shows (for whites) the correlation between exit and voice in different types of neighborhoods and for the two status groups. The significant fact that emerges is the *positive* correlation between exit and voice among high-status residents of the urban and mixed areas and the *negative* correlation among the high-status residents of suburban areas. Evidently, in the urban and mixed areas, the high-status residents most prone to voice are also the ones most prone to exit, while in suburban areas the opposite is the case: the high-status voice-prone population is most likely to stay in the area.

The finding is elaborated in Table 5, in which the respondents who experienced neighborhood problems are classified into four possible response-types: those who choose exit only, those who choose voice only, those who choose both, and those who choose neither. Our concern is with the dominant patterns in the different types of neighborhoods. Propositions 5a, 5b, 6a and 6b summarize the differences between central and suburban areas; the data make it plain that the mixed areas are transitional between these two extremes.

5. *Urban Areas:*
    a. *The dominant pattern among the high-status residents is voice and exit together, with voice by itself a poor second.*
    b. *The dominant pattern among low-status residents is exit by itself, with neither exit nor voice (apathy or resignation) a close second.*
6. *Suburban Areas:*
    a. *The dominant pattern among high-status residents is voice by itself, with voice and exit together a poor second.*
    b. *The dominant pattern among low-status residents is also voice by itself, with the other three far behind. Apathy or resignation is much less frequent than among low-status residents in urban areas.*

Our model raises the question: What are the system-level consequences of the multitude of individual adaptations to neighborhood conditions? We argued that voice—particularly on the part of people with ample political resources—can reasonably be expected to increase the realization of sought-after outcomes. In the suburbs, the dominant strategy of both

Table 5. The Relation Between Exit and Voice among Whites; Percentage of Each Education Group in the Four Response Categories

	Suburban Areas	Mixed Areas	Urban Areas
A. High Educated:			
Response Type			
Exit only	7.1	1.7	5.9
Voice only	62.4	58.6	20.6
Exit and Voice	24.7	28.6	67.6
Neither	5.8	12.1	5.9
Total	100.0%	100.0%	100.0%
	(85)	(58)	(34)
B. Low Educated:			
Response Type			
Exit only	11.1	21.4	40.0
Voice only	55.6	57.1	13.3
Exit and Voice	16.6	7.1	13.3
Neither	16.7	14.3	33.3
Total	100.0%	99.9%	99.9%
	(18)	(14)	(15)

status groups *is* voice; the minority of the low-status population living in the suburbs respond in virtually the same way as their high-status neighbors. Equally important, these "voice-prone" suburbanites apparently intend to *stay* and fight, since only a few of them fall into the "exit-and-voice category. Hirschman's discussion of quality-conscious people fits these residents well: faced with problems in their high-quality neighborhood they must voice in order to maintain their enjoyment of it, since other neighborhoods offer only low-quality options.

On the other hand, the "voice-prone" population of the urban areas is very largely also "exit-prone." Most voice in these neighborhoods comes from high-status residents, but the overwhelming majority of these activists are also people who plan a move *out* of the neighborhood. Our model pointed out that migration from a neighborhood was likely to reduce the area's tax base (since people who have money are likely to use it in making a move to a better neighborhood) and therefore lower its capacity to solve its problems. It also pointed out, borrowing from Hirschman, that the people who make the move will very often be the people who—had they stayed—would have expended the energy needed for neighborhood improvement. This general description seems to fit the urban areas. When high-status people move they are more likely (since they have the money) to move to an entirely different neighborhood, certainly a "better" one and probably in the suburbs. When they leave, they take with them taxable income *and* their political energy as well. We have no way of knowing which is the worst consequence for urban areas, but it is clear that both contribute to perpetuating neighborhood problems there.

Table 6. Exit-Fatigue among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Exit and Voice by Number of Previous Addresses

	Problems and Exit	Problems and Voice
Number of other addresses:		
One	.200 (78) (p<.28)	.434 (76) (p<.09)
Two	.305 (94) (p<.15)	.432 (94) (p<.08)
Three-four	.273 (102) (p<.20)	.461 (99) (p<.06)
Five+	.280 (89) (p<.19)	.238 (88) (p<.22)

Table 7. Exit-Fatigue among Whites; Correlation (Yule's Q) Between Seeing Problems and, Respectively, Exit and Voice by Length of Residence and Urbanism

	Length of Residence		
	−2 yrs	−6 yrs	6 yrs+
A. Suburbs:			
Problems and exit	−.179 (51) (p<.33)	−.425 (43) (p<.17)	.599 (104) (p<.008)
Problems and voice	−.098 (50) (p<.42)	.696 (42) (p<.05)	.212 (105) (p<.27)
B. Mixed areas:			
Problems and exit	.035 (40) (p<.47)	−.172 (41) (p<.37)	.216 (79) (p<.27)
Problems and voice	.679 (40) (p<.03)	.388 (41) (p<.21)	.344 (76) (p<.14)
C. Urban areas:			
Problems and exit	.294 (23) (p<.32)	.724 (14) (p<.06)	.900 (48) (p<.0001)
Problems and voice	.647 (23) (p<.10)	.538 (14) (p<.19)	−.167 (46) (p<.36)

Clearly, living in a suburban neighborhood increases the chances of an individual voicing, and living in the urban areas increases the chances of his exiting. Within such neighborhoods, however, several other factors seem likely to influence his choice of response. One of these is what might be called "exit-fatigue." If a person has made many moves in search of a satisfactory neighborhood, it seems possible that as time goes on he will become increasingly willing to stay and try to make something out of the place where he happens to be; voice, in other words, might increase with number of previous moves, and exit might decrease. A related hypothesis is that exit becomes a more frequent response as length of residence in a particular neighborhood increases, and that voice becomes less frequent. After having paid the costs of a move, people might be unwilling to pay them again for a while and during that time might be more willing to pay the costs of voice. Data relevant to these two hypotheses are given in Tables 6 and 7.

The first hypothesis is not supported: Table

6 shows no relation between the number of other addresses in Columbus and the tendency to exit or to voice. Evidently, the amount of moving one has done *before* coming to an area has no bearing on how one responds to the problems one faces *in the present*. On the other hand, Table 7 shows that length of residence in a neighborhood does have an impact on the strategy one is likely to use, and that this impact varies among types of neighborhoods.

The findings are summarized by propositions 7, 8, and 9:

7. *In urban areas, voice decreases rapidly with length of residence, and exit increases rapidly.*
8. *In the mixed areas, voice decreases with length of residence, but not as rapidly as in the urban areas.*
9. *In the suburban areas, the period between the third and sixth years of residence is a time of lowest exit and highest voice, after which voice decreases and exit increases.*

The hypothesis that people who have just made a move will be unwilling to exit for a while must be modified by the comparative instability of residents of the suburbs and mixed areas during the first two years; in these areas the period of greatest stability is from the third through the sixth year of residence. After an initial testing period during which a new resident "checks out" a neighborhood, if he likes it he becomes more committed. But these first years in the suburbs and mixed areas are, nevertheless, much more stable than the first years in the urban areas. If a period of "checking out" happens in the urban areas, there is no evidence that acceptance is very widespread, since exit snowballs from its already high rate to far higher rates. In the suburbs and mixed areas, "exit-fatigue" seems to have vanished after the sixth year, but in the urban areas it is much more rapidly overcome.

The other part of the hypothesis—that when people are "fatigued" by a recent move, they will be more likely to voice—does gain support from the data. In the suburbs, for example, voice is highest during the stable middle period, and declines during the next period as exit once again becomes acceptable. In the urban areas the two sets of correlations move in opposite directions: during the first period when exit is lowest, voice is highest, and during the last period when exit is highest, voice is lowest. When people are most reluctant to use the exit option, it seems that they are willing to turn to voice to deal with their problems.

Several other hypotheses dealing with influences on choice were tested. The first dealt with sunk costs in the neighborhood. We hypothesized that people who owned a home in a neighborhood would be most reluctant to exit and most willing to voice in the event of problems. This seemed reasonable since a home represents a financial investment that is likely to deteriorate with deterioration in the neighborhood; people with such an investment might be expected to fight in order to save it. Conversely, it seemed possible that exit would be lower among home owners. That this was not the case is shown by Table 8.

The second dealt with what Hirschman calls "entry costs." As he points out, a person who has paid very high costs for entering an organization will be more likely than others to reject exit in the event of deterioration and to try voice in order to keep things as he wants them: ". . . it may be expected that once deterioration is averted to, members of an organization that requires severe initiation will fight hard to prove that they were right after all in paying that high entrance fee."[43]

It seemed quite plausible to hypothesize that the high levels of voice among suburbanites and the low levels of exit were a consequence of such "entry costs." Much of the present suburban population has migrated from the central areas and, in doing so, has paid a high price. Whether or not they own a home, their willingness to stay and fight might be a reflection of their determination not to make another move. In order to test this hypothesis, we drew on the measure of urbanism for the past place of residence. This enabled us to distinguish three categories of respondents: those whose move was

Table 8. Home Ownership and Response among Whites; Correlation (Yule's Q) between Seeing Problems and, Respectively, Exit and Voice by Education

	Home Owners	Renters
A. High Educated:		
Problems and exit	.201 (201) (p<.19)	.358 (104) (p<.08)
Problems and voice	.121 (207) (p<.32)	.649 (105) (p<.006)
B. Low Educated:		
Problems and exit	.538 (75) (p<.06)	.149 (38) (p<.38)
Problems and voice	.179 (72) (p<.30)	.101 (39) (p<.41)

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[43] Hirschman, p. 93.

Table 9. Entry Costs; The Four Responses among Geographically Mobile People*

	Movement Toward Urban End	Movement Toward Suburban End
Rank order		
First	Voice only (58.8%)	Voice only (46.9%)
Second	Exit and Voice (22.1%)	Exit and Voice (37.5%)
Third	Exit only (10.3%)	Neither (12.5%)
Fourth	Neither (8.8%)	Exit only (3.1%)
Total	100.0% (68)	100.0% (32)

* Cases include only those seeing problems.

toward the more suburban end of the urbanism continuum, those whose move was toward the more urban end of the continuum, and those whose move was between essentially similar areas—whether they were urban, mixed or suburban areas.[44] We examined the dominant pattern of responses among those who reported problems in their neighborhoods. The data are given in Table 9. Some evidence in support of the hypothesis comes from the fact that the total incidence of voice is higher among those who made a move toward suburban areas, but the difference is not great and is based on a large number of respondents who both exited *and* voiced after the suburban move. Making finer distinctions among mobility types leaves us with too few cases for reliable estimates, but does not give any evidence in support of the hypothesis, either. Generally, the similar rank ordering of the two broad types suggests that the past residential history of a person has little impact on his mode of response to problems where he lives at present. This finding is consistent with the data showing that the number of moves made before arriving at the present neighborhood has little impact on the choice of a strategy. Our overall conclusion is that the choice of a strategy for responding to neighborhood problems depends—in addition to the factors spelled out in the model—on the type of neighborhood a person lives in and how long he has lived there.

We also tested and rejected the hypothesis that social integration into a neighborhood would make exit less likely and voice more likely. We expected, very simply, that a person who had friends in the neighborhood would be more reluctant to leave than one who knew nobody there; we also expected that he or she would be more likely to be drawn into attempts at remedial action for the neighborhood. We asked the question: "Do most of your friends live around here, or do they live somewhere else?" A few respondents who said they had friends "both here and elsewhere" were coded as "friends here." The data do not support the hypothesis—in fact, they are the opposite from what we expected. They are given in Table 10.

Table 10. Location of Friends among Whites; Correlation (Yule's Q) Between Problems and, Respectively, Exit and Voice by Education and Location of Friends

	Friends Around Here	Friends Elsewhere
A. High Educated:		
Problems and exit	.523 (140) (p<.006)	−.014 (179) (p<.48)
Problems and voice	.255 (141) (p<.22)	.427 (173) (p<.05)
B. Low Educated:		
Problems and exit	.517 (51) (p<.08)	.042 (60) (p<.47)
Problems and voice	.048 (49) (p<.45)	.139 (60) (p<.36)

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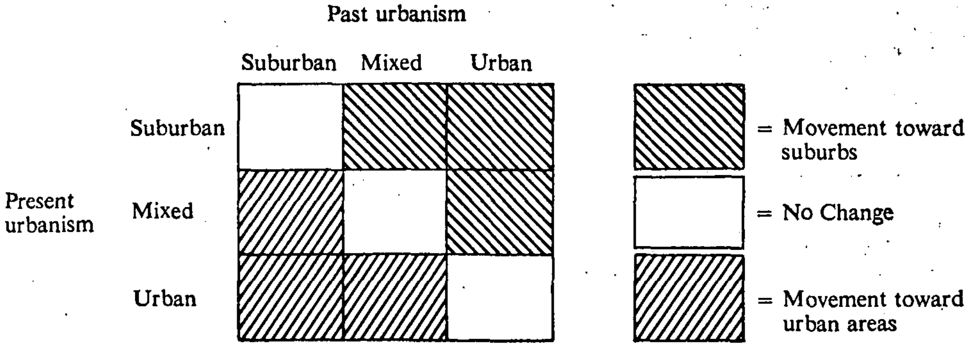
[44] The categories were defined as follows



We have no ready explanation for the propensity of people with friends in the neighborhood to be *more* ready to leave and *less* likely to voice than people without friends there. This is not a function of different friendship patterns in different neighborhoods since holding urbanism constant makes no major difference to the pattern. We can, however, draw the limited conclusion that:

10. *Social integration into an area does not make one more likely to stay and fight when problems there arise.*

It seems that if a local community of "friends and neighbors" was ever the basis for political action in American cities, it is no longer so. We suspect that easy access to all parts of the city reduces the importance of living close to one's friends, so that community, while it has not ceased to exist, is no longer readily defined in geographic terms.[45]

Our conclusion for whites can be summed up as follows: While status (defined by education) makes some difference in selecting a strategy independently of area characteristics, other individual-level differences (home ownership, integration in the neighborhood) make no difference. The most important factor seems to be the type of neighborhood one lives in and the length of time one has lived there.

B. Blacks:

As we pointed out earlier, blacks experiencing problems in their neighborhoods face a fundamentally different situation from that of whites experiencing problems. In many respects their situation is similar to that of a purchaser who is obliged to buy his goods from a monopolistic enterprise. Hirschman's hypothesis is that a monopoly situation is likely to breed voice, since voice is the only available way of resisting decline in the product:

> The actual level of voice feeds on inelastic demand, or on the lack of opportunity for exit . . . [In this situation] the role of voice would increase as the opportunities for exit decline, up to the point where, with exit wholly unavailable, voice must carry the entire burden of alerting management to its failings.[46]

Of course, individual blacks do have opportunities for movement out of the ghetto, but segregation effectively constrains the movement of most in the population. Blacks are obliged to "purchase" their residential neighborhood in a severely restricted market.

Nevertheless, the potential of the voice mechanism for blacks is also notoriously limited. We have already cited Parenti's case studies showing the ineffectiveness of attempts to make modest changes in certain ghetto neighborhoods in Newark.[47] Many commentators make the same point. The important question is: Given these limits on both exit *and* voice, what do central city blacks *do* in the face of neighborhood problems?

Because of the small number of cases, our data on blacks must be taken with reservations;[48] nevertheless, they are highly suggestive. The first proposition that gains support is:

11. *Blacks are more likely to voice in response to problems than are whites of similar status who live in similar urban areas.*

Table 11 shows the distribution of response types among blacks who reported problems in their neighborhood; the corresponding data for whites are contained in Table 5. The striking difference is the comparative frequency of voice *by itself* among blacks: 40.0 per cent of the more highly educated blacks take this option compared to 20.6 per cent of highly educated whites living in urban areas, and 35.7 per cent of less educated blacks compared to 13.3 per cent of less educated whites living in urban areas. Our data are thus consistent with Olsen's earlier mentioned finding that when status is controlled, blacks are now participating in social action more than whites.

We also pointed out that the high frequency

Table 11. Relation Between Exit and Voice Among Blacks; Percentage of each Status Group in the Four Response Categories by Education*

	High Educated	Low Educated
Response Type		
Exit only	5.0	14.3
Voice only	40.0	35.7
Exit and Voice	45.0	28.6
Neither	10.0	21.4
Total	100.0	100.0
	(20)	(14)

* Cases include only those seeing problems.

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[45] A similar point has been made by Stanley Milgram in his discussion of adaptations to overload in metropolitan areas. See his "The Experience of Living in Cities."

[46] Hirschman, p. 34.

[47] Parenti, "Power and Pluralism."

[48] Blacks are about 13.3 per cent of the population in the Columbus SMSA. Our sample of the area included 13.9 per cent blacks. Because we did not over-represent blacks, we have only 85 cases to deal with. Care was taken to ensure that black students did the interviewing in black neighborhoods, and white students in white neighborhoods.

of voice and exit together among high-status white residents of the urban areas was likely to drain those areas of political energy that might help toward recovery. Nathan Wright has argued that among blacks, a similar departure of middle-class residents from the urban areas represents an "incalculable loss politically."

> . . . [this departure] has represented a hazardous drain upon a resource crucial for the new civic, political, and economic adjustments the cities have to face. . . . While other rising ethnic groups built power blocs that gave them a voice in the determination of civic, political, and economic affairs, black men and women possessed of the greatest leadership possibilities followed the path of flight to the suburbs.[49]

Our data (admittedly scant) suggest that the problem *is* present among high-status blacks, but it is not as crucial as for whites: only about half of the voicing population is also exit-prone. Evidently, the financial resources that make exit the easiest and best strategy for many high-status whites in these areas do not function in quite the same way for high-status black residents of the ghetto. This is consistent with the hypothesis that constrained opportunities for exit—such as face blacks in most American cities—result in heightened voice. Although the data are weak, we can speculate that this fact makes the prospect for solving problems better in the ghetto than in the surrounding white areas, assuming the problems are of equal severity.

With low-status blacks as well as high-status blacks, the *total* voicing population is far larger than among whites of similar status: together the "voice only" and "voice and exit" populations constitute 64.3 per cent of the low-status black population compared to 26.6 per cent of the low status white population in the urban areas. Once again, though, about half of these "voice-prone" blacks are also "exit-prone." The *comparative* proneness to voice among ghetto blacks provides a firm basis for attempts to organize the black community into effective political action, but the main obstacle to such attempts appears to be the *temptation* of exit—despite the fact that residential moves by blacks are almost always within the limits imposed by the ghetto.[50]

Table 12. Correlation (Yule's Q) Among Blacks Between Seeing Problems and, Respectively, Exit and Voice by Length of Residence

	Length of Residence		
	−2 yrs	−6 yrs	6 yrs+
Problems and exit	.250 (17) ($p<.36$)	.909 (20) ($p<.0001$)	.366 (36) ($p<.23$)
Problems and voice	† (16)	.362 (20) ($p<.27$)	.546 (37) ($p<.07$)

† The distribution of cases in this cell made the Yule's Q meaningless.

Table 12 shows, for blacks, the relation between seeing the neighborhood's problems and, respectively, exit and voice when length of residence is held constant. The trend it reports is almost the opposite of that for urban whites (see Table 7). Among whites in these areas, exit increases rapidly with length of residence and voice decreases rapidly, but:

12. *Among blacks, exit reaches a peak a few years after arrival but declines thereafter.*
13. *Among blacks, voice increases with length of residence.*

The earlier data showed that, among whites, exit during the first two years of residence was more frequent in the urban areas; evidently, the "fatigue" arising from a recent move is not sufficient to stop white residents of urban areas from planning another move soon after arrival. Ghetto blacks are similar to urban whites in this respect and similar also in the rapid increase of exit during the next few years. They are different, however, in the decline of exit beyond that and in the increase of voice. It is as if the realities of constrained exit opportunities are finally driven home by repeated disillusionment with "new" neighborhoods. When a decision is finally made to stop looking, it seems that a parallel decision is also made to start fighting.

For both blacks and whites we tested the hypothesis that age would make a difference in the selection of a strategy; we expected that older people would have "deeper roots" in their neighborhood and, because of that, would be less likely to exit and more likely to voice. Table 13 breaks respondents into two broad age groups—above 35 and below 35. It shows the correlation between seeing the problems and, respectively, exit and voice when race and age are taken into account.

In nonurban areas there was little difference attributable to age, and Table 13 includes only whites living in urban areas. Perhaps the slight

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[49] Nathan Wright, *Ready to Riot* (New York: Holt, Rinehart and Winston, 1968), pp. 60–62.

[50] As expected, our data show that virtually all black residential mobility does take place between areas at the extreme "urban" end of the urbanism continuum.

Table 13. Effect of Age; Correlation Between Seeing Problems and, Respectively, Exit and Voice By Race and Age

	Below 35 Years	Above 35 Years
A. Whites		
Problems and exit	.149	.337
	(157)	(290)
	($p<.26$)	($p<.05$)
Problems and voice	.347	.260
	(153)	(288)
	($p<.13$)	($p<.08$)
B. Blacks:		
Problems and exit	.684	.307
	(20)	(53)
	($p<.10$)	($p<.22$)
Problems and voice	.905	.421
	(20)	(53)
	($p<.0001$)	($p<.11$)

differences among whites in these areas suggest that with increasing age the preference for exit over voice that we observed at all ages increases. The most interesting age comparisons, however, are among blacks:

14. *Younger blacks are substantially more prone to exit and to voice than older blacks.*

Young blacks are more prone to voice than they are to exit, but the most characteristic thing about them is that they are very likely to do *either*. Our data support the standard observation that the restlessness of the ghetto at present is largely a restlessness of youth. But they elaborate that observation by showing this restlessness to be geographic as well as broadly "political."

The problem for those organizing black political action is to overcome this residual tendency to exit in the face of bad neighborhood conditions. This tendency can be reduced by making exit still less attractive or by making voice *more* attractive or both. For collective action to be sustained or even to get started, it must have reasonably good prospects for succeeding, a condition that rarely holds for ghetto blacks. Lipsky argues that evidence of success is necessary to sustain a protest movement,[51] but if there is no such evidence then reliance may have to be placed on charisma and other "nonpolitical" appeals. It seems likely, though, that the increasing black dominance of central city politics will change such assessments. Newark, where Parenti found such high levels of cynicism about political effort, now has a black mayor, and so do several other cities. If exit remains a limited strategy for blacks—and there is evidence that it will since segregation in housing appears to be *increasing*—then these changes may cause voice to increase to unprecedent levels. In short, segregation *plus* viable voice opportunities seems one recipe for massive political action.

Implications

Figure 1 is a decision-tree showing the various paths to voice, exit and passivity. Notice that, for purposes of simplicity, we have made the exit decision come first in the sequence, but there is no necessary priority of this decision over the voice decision; we only suggest that both decisions are made at some time by people experiencing problems. Notice also that there are two paths to each of the three possible behaviors. One can get to passivity, for example, either because there are no problems experienced or because there are no solutions perceived.

Our main interest, however, is in the patterns that seem characteristic of different kinds of metropolitan neighborhoods. We have identified three such patterns, remembering that we are talking only about central tendencies. The first is the sequence for blacks in the ghetto: if problems are experienced, exit is judged impossible, and voice is judged possible. An earlier set of political realities, we suspect, meant that voice was also judged impossible, resulting in terminal passivity for most blacks. The second sequence is for urban whites: if problems are experienced,

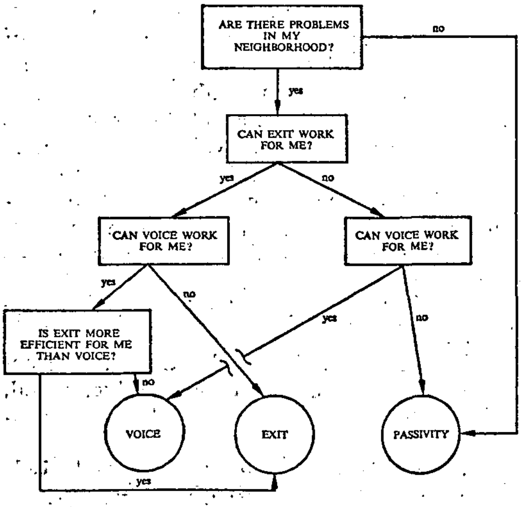

Figure 1. Decisions leading to exit, voice and passivity.

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[51] Lipsky, "Protest as a Political Resource."

exit is judged possible and voice either more problematic or impossible resulting in terminal exit. The third sequence is for suburban whites: if problems are experienced, exit is judged possible and voice is also judged possible. We have suggested that the decision between exit and voice is most likely to be decided by such considerations as quality consciousness, the absence of still more attractive neighborhoods to move to, and the viability of political processes. Like ghetto blacks, but for very different reasons, suburban whites decide to stay and fight.

The important difference is in feedback to neighborhood conditions themselves. Magoroh Maruyama has made a distinction between deviation-counteracting and deviation-amplifying social processes.[52] Traditional cybernetics, he argues, has been mainly concerned with the former—processes whereby a system of interacting parts moves toward some stable equilibrium. "The second cybernetics," as he calls it, deals with processes that move a system of interacting parts away from such a stable equilibrium. In some situations, "whatever the change, either an increase or a decrease, amplifies itself."[53] In suburban neighborhoods, problems characteristically produce voice, which, according to our model, contributes to solving those problems. As the problems disappear, people are increasingly freed from the need to decide between exit and voice and can sink into happy passivity. Problems produce responses that eliminate the problems. In urban areas that are white, on the other hand, the appearance of problems triggers exit which, we have argued, only makes things worse by draining the area of its tax base and its political energy. This is clearly a deviation-amplifying process: problem solving in these areas makes problems there get worse and worse.[54]

The interesting question about ghetto neighborhoods is this: will voice continue to be seen as a viable option? We have suggested that this depends, to some extent, on the success blacks have in gaining control of central city political institutions. But it also depends on the availability of resources sufficient to do something about ghetto problems. Without such resources, we can well project a future in which blacks once again lapse into political passivity. This time it would be based on actual experience with the futility of voice, and we can expect that the attendant frustrations would be significantly greater than now. But this is to relax our assumption that voice is likely to improve things, and without projecting to such a depressing future, we seem to have identified another deviation-counteracting feedback process: problems in the ghetto produce adaptations that make problems there get better and better.

The question still remains to be answered: *What* problems are being dealt with by the residents of the different neighborhoods? Table 14 reports the percentage of all respondents mentioning a problem and the mean number of problems they mentioned in the three types of neighborhoods and in the black ghetto. Although, as we might expect, whites in urban areas mention somewhat more problems—and more of them mention problems—than whites elsewhere, there are not major differences by neighborhood. Problem-awareness is fairly constant in all areas, it seems.[55]

On the other hand, the actual problems mentioned differ very sharply among types of neighborhood. Table 15 reports the problems that were mentioned first by those respondents who did mention something; it gives the percentage distribution of all problem mentions in the three types of neighborhood and the ghetto.

Table 14. Concern about Problems in Different Types of Metropolitan Neighborhoods

	Suburban Areas	Mixed Areas	Urban Areas	Blacks
Percentage mentioning any problem	50.2	42.9	60.2	44.2
Mean number of problems mentioned	.79	.70	.83	.72
Total population	213	177	93	86

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[52] Magoroh Maruyama, "The Second Cybernetics: Deviation-Amplifying Mutual Causal Processes," *The American Scientist*, 51 (1963), 164–179.

[53] Maruyama, 177.

[54] Hirschman makes a similar point (p. 122) by classifying "inner cities" as "organizations" that are primarily sensitive to voice but in which decline in performance arouses mainly exit.

[55] Because urban residents now tend to be less educated than suburban residents, we should expect that a greater proportion of suburbanites would mention more problems; education is usually associated with articulate and full responses to questions such as these. But in the suburbs the two education groups are just about the same in this respect: 54.0 per cent of the more highly educated mention problems and 50.0 per cent of the (minority) less educated population do so. The predominance of more educated people in the suburbs does not inflate the aggregate number of problem mentions.

On the other hand, educated people are more articulate when they have something to talk about: 69.2 per cent of the more educated white respondents living in urban areas mention one or more problems,—i.e., more than their status counterparts in the suburbs—while only 54.3 per cent of the less educated respondents do.

Table 15. The Percentage Distribution of Problem Mentions, by Urbanism and Race

	Suburbs	Mixed Areas	Central Whites	Central Blacks
Problem mentioned:†				
Environmental quality	4.8	19.2	21.7	17.8
Crime	22.5	23.4	30.4	32.1
Schools	24.2	17.0	0.0	7.1
Transport	29.0	14.9	8.2	10.7
Slums/housing	6.5	12.8	39.2	28.5
Race related	12.9	12.8	0.0	3.6
Total mentions (1st item)	62	47	40	28

† Coding: Environmental quality included such things as air pollution, water pollution, beautification in general.
Crime included such things as police problems, youth violence, street lighting, lack of safety on the streets.
Schools included such things as their general inadequacy, the need for more and better schools, discipline problems.
Transport included such things as the inadequacy of public transport systems, their cost and poor coverage, difficult access to other parts of the city.
Slums/housing included such things as the need for slum clearance, the problems from slum clearance, high rents, the difficulty of finding a house.
Race included such things as blacks moving in, the dislike of proximity to blacks, racial difficulties in general.

These data are best interpreted as giving the *priority* of concerns in different areas. The greater frequency with which schools are mentioned in the suburbs supports the general thesis referred to earlier about the familistic concerns of suburbanites, but the fact that the schools are not mentioned by urban whites does not necessarily mean they are not seen as a problem by them. It does mean that other things—such as housing problems—have a greater urgency and are mentioned first. Only in matters related to crime do the residents of all areas have nearly equal concern; although police records show crime decreasing precipitously from the central areas outward, suburbanites nevertheless feel a threat from crime that is comparable to the threat felt by urbanites.[56]

The important matter is the correlation between the problems experienced by the residents of different neighborhoods. As expected, suburban whites and urban whites differ sharply: the rank-order (rho) correlation between the kinds of problems mentioned in the two areas is −.42. On the other hand, the correlation between the problems reported by whites and blacks in the central areas is nearly perfect. Whites who live in the suburbs and the central areas have very different concerns, but the blacks and the whites who live in neighboring areas of the central city have virtually identical problems to deal with.

Earlier, we drew on Hirschman's suggestion that many problems, and complex ones, might make voice an inefficient mechanism. Banfield has pointed out that:

> For something like two-thirds of all city dwellers, the urban problems that touch them directly have to do with comfort, convenience, amenity, and business advantage . . . these are "important" problems but not "serious" ones. In a great many cases, these problems cannot even be called important; a considerable part of the urban population—those who reside in the "nicer" suburbs—lives under material conditions that would be hard to improve upon.[57]

The present data support our suspicion that suburban problems are "easier" to deal with, and therefore more likely to provoke voice; problems such as environmental quality, crime (the *reality* of crime), and poor housing are likely to be more "difficult" for the urbanites who experience them than are the transportation and school problems that plague suburbanites.

One intriguing prospect is that the neighborhood problems of central city whites might be reduced by the voice behavior of ghetto blacks. We have shown them to experience similar problems, and we have also shown that, while whites characteristically try to leave the area to make things better for themselves, blacks characteristically take some kind of action; we drew the inference that black voice was likely to increase greatly in the near future. Earlier analysis of the present data has shown that low-status white hostility toward blacks is greatest when they live close by;[58] one plausible projection to the near future is that the neighborhood problems of these central city whites might be ameliorated by spillover from the political activity of neighboring blacks—in short, by the Black Power movement.

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[56] Thus, in the urban areas, but not in the suburbs, there does appear to be a sound base for "law and order" as an issue. The Columbus City Police keep unusually good records of the incidence of crime by census tract. See *1965 Statistical Supplement to the Annual Report of the Columbus City Police Department.*

[57] Banfield, *The Unheavenly City*, pp. 11–12.

[58] John M. Orbell with Kenneth S. Sherrill, "Racial Attitudes and the Metropolitan Context; A Structural Analysis," *Public Opinion Quarterly*, 33 (Spring, 1969), 46–54.

Although rational grounds appear to exist for a political alliance between ghetto blacks and central city whites, the realities of racial hostility are likely to hinder its realization. Perhaps the political parties, particularly the Democratic party, might provide an organizational basis and an arena for conflict to the benefit of both races, but this possibility depends on the extent to which the parties are accepted by blacks as useful.[59] A hindrance of a different kind, however, might be the disinclination of central city whites to engage in political action *at all.* The question we must ask is: What changes would increase the willingness of whites in the central city to take corrective action in their *present* neighborhoods? Quite obviously, it is impossible to make major reductions in central city exiting behavior, for exit is just too easy and readily available a solution to what is bothering people. But some modest interventions might make equally modest changes that might have, in turn, significant consequences. One logical possibility is to increase the costs of entry into the suburbs, thereby making it more rational to stay in the present area and fight. Higher suburban taxes, for example, might have this effect. But in general, we might anticipate that such tactics would only limit the movement of those anyway least able to make the move; those with financial resources (whom we need in the city) would be able to overcome this barrier.

A second and more reasonable possibility is to increase the prospects for successful voice in the central areas. Richard Sennett, among others, has mourned the passing of the political machine as a device for linking individual to political decision making:

> For insofar as the reform crusades have succeeded in amending the past, the little guys, the white working-class and lower middle-class voters, have lost their sense of connectedness with the body politic; when the machines lost a personalistic character, the little guys were isolated from the only channels of political influence *they* believed effective. The "alienated voter" phenomenon is not a slogan; there is . . . a real and powerful sense of disconnection. There is now a large debate going on in political science circles over whether in functional fact the little-voter is so cut off from power. From the human point of view, this debate is academic—the people themselves feel cut off, perceive themselves as robbed of something they cannot put a name to.[60]

While the machine in its traditional form is, no doubt, gone for good, it might be possible to restore some of this "connectedness" that it once supplied. Neighborhood control of schools (not a *primary* source of concern from our data, but an important one) has been attempted in various places with mixed results; one obvious consequence has been to heighten the level of conflict in the community where it is tried. But a high level of conflict is one indicator of active voice, and is not—in itself—necessarily undesirable.[61]

In order to increase voice in these white urban areas, it is necessary—from our model—to give people grounds for believing that their efforts have a *reasonable* chance of influencing for the better the things they are concerned about. One of the most urgent tasks for Political Science is creative theory building about institutional changes that might justify these beliefs.

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[59] For differing views on the propriety of blacks working through the parties, see: Joyce Gelb, "Blacks, Blocs & Ballots," *Polity*, 3 (Fall, 1970) 46–54; James Q. Wilson, "The Negro in Politics," in *American Ethnic Politics*, ed. Lawrence Fuchs (New York: Harper and Row, 1968) pp. 217–246.

[60] Sennett, *The Uses of Disorder,* pp. 79–80. A similar conclusion is supported by quantitative analysis of beliefs about the distribution of power in the United States: the poor and Negroes give most credence to elitist and economic models of power, while the pluralist interpretation is supported most by high-status people. See William H. Form and Joan Rytina, "Ideological Beliefs on the Distribution of Power in the United States," *American Sociological Review,* 34 (February, 1969), 19–31.

[61] To the contrary, Sennett has argued that individuals whose environment exposes them to conflict and obliges them to deal with it in a realistic way are unlikely to resort to violence in time of major crisis.

Some Conceptual Problems in Nuclear Proliferation

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The Proliferation Problem

A world in which many more countries acquire nuclear weapons could be a much more grim one. Wars that today kill thousands might instead kill millions; wars that now are averted might instead be launched, simply because each side preemptively felt it had such powerful weapons that it dared not hesitate to use them.

To prevent a further spread of nuclear weapons, the United States and the Soviet Union have offered for signature a Nuclear Non-Proliferation Treaty (NPT) freezing the number of states possessing such weapons at the current five—the U.S., USSR, Great Britain, France, and Communist China.[1] Countries possessing nuclear weapons pledge themselves under the treaty not to give them away or to assist other nations to produce them. Countries not yet having such explosives pledge themselves in the same treaty never to accept them and never to manufacture them. The latter "nonweapons" states moreover would agree to accept inspection safeguards by the International Atomic Energy Agency (IAEA) on all their peaceful nuclear activities, to ensure that such facilities and materials are not diverted to producing nuclear explosives.

The United States and Soviet Union came to agreement on an NPT text between the fall of 1967 and spring of 1968, and the treaty was offered for signature on July 1, 1968; ratifications by 43 states, including the U.S., USSR, and Great Britain, were required for the treaty to go into effect. Acceptance of the NPT was delayed somewhat in the aftermath of the Russian intervention in Czechoslovakia, but on March 5, 1970, Soviet and American ratifications were deposited together with those of enough other nations to activate the treaty. Among nonweapons nations with significant nuclear industries, only Canada and Sweden thus far have signed and ratified the NPT; West Germany, Italy, India, Japan, Australia, Israel, Brazil, and Argentina are some of the other states potentially capable of producing nuclear weapons.[2]

At first glance, we might interpret any foreign resistance to the NPT as indicating a malicious craving for nuclear weapons. More honorable and acceptable arguments can be advanced for opposing the treaty, however—arguments which will induce real political debate in the countries involved. Even if a nation has no intention of ever producing nuclear weapons, it might relish the legal *option* of producing them, for the political concessions this can produce, or simply for the prestige of being on an equal legal footing with the "superpowers." Surrendering the weapons option can also have economic costs. Scientists have argued that valuable spinoffs in the peaceful application of atomic energy may be lost if weapons research is rigorously and explicitly forbidden, since the technologies often overlap. More important, there are definite costs in the verification process which the IAEA would install to assure outside powers that materials were not being diverted to weapons. Nations being inspected will have to detail trained and expensive personnel to accompany IAEA inspectors on their rounds. In some cases existing equipment might have to be redesigned to make adequate monitoring possible. At other times more detailed and costly bookkeeping procedures will be required. At the extreme, it might even be necessary to shut down operations in some facility to confirm records on the fissionable materials it contains.[3]

States allowed to retain nuclear weapons under NPT may openly or secretly welcome the binds it imposes on the countries which never get into the "nuclear club." States denied such weapons will resent such binds on themselves, but may welcome the bind on other "nonweapons" states. The debate on the NPT, or on alternative barriers to the spread of nuclear weapons, will thus take varying forms from country to country. National values will be crucial to the trends of such debates, as will the particular political and economic situations of the countries involved. Technical opinions will also be crucial, as each society consults its physicists on the priorities and opportunities involved, asking such questions as: "If we retained our legal option to make bombs, how long would it take us to acquire them?" "How far are our neighbors from the bomb?"

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[1] The text of the Nuclear Non-Proliferation Treaty as finally presented can be found in United States Arms Control and Disarmament Agency, *Documents on Disarmament: 1968* (Washington: U.S. Government Printing Office, 1969), pp. 461–465.

[2] A full discussion of the spreading potential for nuclear bomb production, and efforts to control the spread, can be found in C. F. Barnaby, ed., *Preventing the Spread of Nuclear Weapons* (London: Souvenir Press, 1969).

[3] A basic analysis of the costs and problems of inspection appears in Arnold Kramish, "The Watched and the Unwatched," *Adelphi Papers* (Institute for Strategic Studies, London) 36 (June, 1967).

Yet authoritative answers here can easily be misunderstood within a conceptual confusion on "how close" nation X or nation Y is to making an atomic bomb. Depending on context and political biases, clearly different answers will be supplied in response to seemingly identical questions. The intent of this article is thus to explore the semantic ambiguity which appears in physicists' answers to politicians' questions on proliferation.

If we ask how far Japan is from the bomb, we might get an estimate of "three years." This answer does not tell us that Japan will have a bomb in three years; yet neither does it assure us that three years from now Japan would still have to wait three years for a bomb. Perhaps three years is the minimum required if a deliberate effort is expended; yet if no explicitly military effort is expended, will the time-lag remain the same?

What will concern most of us is not the earliest date at which six or eight countries can have bombs; there may be no evidence of crash programs in any of these states. Rather we must fear what the time-lag for any hypothetical deliberate crash program will asymptotically sink to, simply as the result of the more innocent civilian-purpose steps already being taken in each country. The following diagram is intended to clarify some of the distinctions involved here:

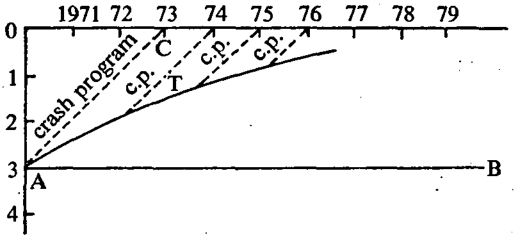

Figure 1. Innocent progress toward the bomb.

The vertical axis indicates the number of years it will take some nation to produce nuclear weapons of Nagasaki magnitude if it begins at any particular date. The horizontal axis specifies, on an identical scale, years of the future for which the above scale applies. AT illustrates what may be a typical curve. By the very definition of the axes, no curve can approach the zero time-lag line more rapidly than at a 45 degree angle (AC). If Sweden were defined as being four years from the bomb and started her crash program now, she in four years would be zero years from the bomb. Any 45 degree line, any line parallel to AC, is a crash program line. (For the moment, we are only discussing time-lags to production of *bombs;* if delivery systems are judged important, these can be added to the analysis as suggested later in this article.)

The concept of a "crash program" may be a little fuzzy. Political decision-making bodies will typically see certain "satisficing" points[4] at which they are doing "all that can be done" to secure some weapon. From our disinterested perspective, we might see this as a rationalization; perhaps one can *always* achieve still more rapid results if one only will spend still more money; perhaps not. The lengths of time assumed for "crash programs" here thus reflect the probable willingness of states to devote resources to the race, once an explicit move toward nuclear weapons has been approved.

Why are the questions posed on such a graph relevant? Why indeed should the outside world care *how far* any particular state is from acquiring nuclear weapons? We of course prefer that this state not have such weapons, so we hope that it will be *some* distance from them. If this distance is a little greater, moreover, we can expect to detect any deliberate move to acquire bombs before it is successful, while we still will have time to initiate political, economic, or even military retaliatory moves. We hope that anticipation of such detection and retaliation will deter the move for the bomb in the first place. The longer the time-lag until a bomb could be produced, the more time for the outside world to notice and react to such a move, and the greater the deterrent to it. In the worst or tensest situation, the sanction for any perceptible grasping for bombs might be war and nuclear attack. Sanctions far short of this might suffice, however, as with general political condemnation by the outside world, the freezing of assets and remittances, or the imposition of some sort of embargo.

The Shrinking Time-Lag

One might now venture to contemplate the characteristics of the normal curve (AT). Why does it not stay horizontal and parallel to the zero line (AB); why does the time-lag not stay constant for a nation not actively seeking to acquire nuclear weapons? A number of factors can be predicted here to bring even the most innocent nations ever closer to the bomb.

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[4] The "satisficing" concept is of course drawn from Herbert Simon, "A Behavioral Model of Rational Choice," *Quarterly Journal of Economics* 69 (February, 1955), 99–118.

Economic needs for electrical power may encourage or require the building of power reactors. Some power reactors are fueled with natural uranium, which is freely available in a number of countries, precluding any external inhibitions to supply. Others are fueled with enriched uranium, which, although it must typically be purchased from a source like the United States, may lend itself more easily than natural uranium to further enrichment to weapons-grade U-235. Either of such reactors produces plutonium as well as electrical power. Plutonium, when the economics of power production justify it, can be processed again in a chemical separation plant for use as fuel for various kinds of reactors. Over time larger quantities of plutonium or enriched uranium will thus come into circulation in various countries. National plants for the production of plutonium or enriched uranium will also become commercially advisable. Plutonium and enriched uranium both can be adapted for use in a bomb.[5]

Quite independently of this production of fissionable materials, basic research may be undertaken on how such material would be assembled into bombs. This may not be inspired by any direct desire to manufacture weapons, but merely by an interest in "pure research"; or it may follow incidentally from research in related practical civilian fields such as controlled fusion or breeder reactors. The need for possible civil defense measures against other nation's bombs may similarly stimulate such research; so will simple patriotic concern that other nations are undertaking similarly basic enquiries. (Thus, for instance, in Sweden basic research has been done on bomb designs although atomic bombs have never been assembled.) If the laboratories required for these investigations are small enough, their activities may be impossible for outside agencies to monitor, and to verify their findings may require nothing so momentous as an actual test detonation.

In general the discoveries of other nations in the nuclear field will steadily leak out, often in straightforward academic publications. Peripheral equipment similarly will become available in world markets at lower costs, perhaps even in cases in which the entire kit would previously have had to be constructed indigenously. Finally, the infrastructure of trained personnel and functional organization will tend to grow in most nations so that any program as sizable as weapons development will be more feasible.

At first, the AT curve may be almost parallel to an AC military crash-program curve, since all the bomb-relevant basic research that could be undertaken will emerge almost naturally in any event. Abstractly, the United States in 1900 was doing all it could do (was on a "crash program") to produce nuclear weapons. So is Upper Volta today. At such an early stage, almost no additional money could be profitably spent to accelerate a bomb capability. Governments and legislatures thus are able to speak of "no need yet to make a decision" on nuclear weapons. All that can be done is being done. This may merely evade a decision for the early years of a nation's nuclear development, but a need for choice definitely emerges later, when the nation must deliberately decide whether an optimal military program will be undertaken or avoided. Yet if such a program is not undertaken, the remaining effort required for bomb-production may still always be decreasing.

One can argue that this menacing overlap between civilian and military nuclear technology is nothing new, for responsible officials have been alert to it for years. No one expects the IAEA inspectorate, which will have primary responsibility for inspecting peaceful nuclear installations under NPT, to spend most of its time watching for autonomous and explicitly military programs; the focus of attention will instead be on the civilian activities which in their natural growth more ever nearer to the military line. Any reader of the evolving IAEA safeguards documents since 1961 will be impressed by their ingenuity in closing all major gaps through which materials might slip unnoticed into bomb manufacture.[6]

IAEA, based in Vienna, came into existence in 1959 and since then has engaged in a mixture of inspection and international technological assistance activities. Inspection has involved materials and equipment which might otherwise be utilized for the production of nuclear explosives; IAEA practices have required access to records on the utilization of such materials, as well as verification of records by inspection of the fuel or equipment itself. IAEA inspection experience has been accumulated under three circumstances to date: when the Agency has itself arranged for the transfer of equipment or fuel, when a technical-assistance agreement between two states has designated IAEA as the inspection agent in place of the donor state, and when a state has unilaterally asked the Agency to apply safeguards over some or all of its facilities.

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[5] Good basic accounts of the overlap of military and peaceful nuclear technology can be found in Leonard Beaton, *Must the Bomb Spread?* (London: Penguin Books, 1966), and Alastair Buchan, *A World of Nuclear Powers* (Englewood Cliffs, N.J.: Prentice-Hall, 1966).

[6] Comprehensive accounts of the evolution of IAEA practice can be found in Arnold Kramish, *The Peaceful Atom and Foreign Policy* (New York: Harper and Row, 1963), and Lawrence Scheinman, "Nuclear Safeguards, the Peaceful Atom, and the IAEA," *International Conciliation*, 572 (March, 1969).

If bomb capabilities are coming a little closer now, the addition of an international inspection or safeguards system can still serve to alert the outside world after a deliberate bomb program has been launched and before it reaches fruition. After completion, it may be more difficult for the outside world to impose any retaliatory sanctions. Indeed, after the bomb program has reached fruition, the nation involved may want to publicize rather than to hide it.

Yet this IAEA vigilance may not relieve our concern but only prove how well taken it was. In effect the ingenuity of IAEA safeguard documents illustrates the running of a possibly hopeless race. The inspectorate must watch what is legitimate, to be certain that nothing illegitimate (i.e., lacking civilian purpose, moving toward explosives) is undertaken. As more and more activities become legitimate—not only reactors, but reprocessing plants, separation plants, and perhaps soon uranium diffusion plants—the inspectors must follow along, watching always a new zone of legitimacy, to make certain that the next "illegitimate" zone is not entered.

As projects justifiable in civilian terms bring nations ever closer to nuclear weapons, the time-lag will continually be shrinking. And even with the presence of an IAEA inspectorate, the outside world may find it impossible to detect a bomb program early enough for meaningful reprisals. Perhaps the IAEA could not even disclose a bomb program as early as the country itself wanted it disclosed; past a certain point, a nation will want to advertise and proclaim the fact that it has decided to violate its NPT commitments.

Questions also can always arise about whether we really know with certainty how far Israel or Canada or Sweden is from a bomb. Without inspection, the extent of militarily useful nuclear progress in a country may be difficult to determine; even with such inspection, a certain percentage of the fissionable material in an advanced commercial system might be undetectably divertable to military uses.

Zero Time-Lags

One might then question whether the AT curve will inevitably reach the zero time-lag line, or whether it will level off at some relatively even distance from it. Perhaps by the very definition of the curve, some gap would always have to remain, for the AT curve describes progress where there is no intention to manufacture nuclear explosives. One can theoretically conjure up a reactor design that would be exactly identical to a bomb, but this happily is unlikely, so that an absence of intent to assemble bombs would presumably keep them from coming into existence. There will thus always be some final work to be done, some final questions to be answered, before bombs would be available. Perhaps three months is a reasonable estimate of the residual time-lag at which we would asymptotically level off; this may be too pessimistic or too optimistic.

A nation may thus wait "just short" of nuclear weapons, as the result of its full development of peaceful nuclear industry. Yet much will depend on how it waits. Some states might elect to allow plutonium to accumulate first, before designing and building any models or prototypes of bombs; this would allow honest disclaimers that no "work on bombs" was under way. Others might move right ahead with prototypes, so as to speed the final production of bombs whenever the international situation seemed to require it. Perhaps India is in the first category today, and Israel may be in the second. Once work on bomb design has been done, however, it cannot easily be undone, as scientists do not quickly forget what they have learned. If a computer simulation has once been run on the design of a bomb, the country which has commissioned it has been moved that much closer to possession of bombs; for an indefinite time into the future, the time-lag until actual weapons production may have been reduced from months to a matter of weeks.

With the necessary margin of explicitly military activity so slight, the temptation to begin and complete a crash program might become very great, especially for nations which have not signed a treaty renouncing nuclear weapons. The mere desire for scientific tidiness may stimulate the manufacture of the bomb, to prove out all the speculation as to whether it could now be easily done. Fears of some preemptive intervention by a hostile state (perhaps one also immeasurably close to bombs) might similarly stampede a nation into crossing the line (see ATN, Figure II); "ten bombs in a cave" may be a better deterrent to war than "ten bombs anytime, at twelve weeks' notice."

If such reasoning on preemptive war is at all correct,[7] one could even question whether peace is best served by trying to hold back nations at such a short distance from bomb stockpiles. Nations with no imminent conflicts with their neighbors may indeed be better off never having touched the zero-line. Those under the shadow of attack might behave less nervously, however, if the line had already been crossed. If assured second-strike forces are preferable to preemptible forces among the great powers, they might also be preferable for some small-scale confrontations.

Legal Obligations

One should thus consider the impact of the obligations imposed by the Nuclear Non-Proliferation Treaty, as well as by other international agreements. NPT commits a nation not to make nuclear weapons, and to accept international safeguards to ensure that it is complying. A nation may withdraw from NPT upon giving three months' notice.

Perhaps more binding still are the various bilateral agreements whereby nations accept equipment or nuclear fuels from abroad, under the agreement that such resources will not be used for military purposes, and again that their use will be subject to international safeguard. Even after three months' notice, such resources cannot legally be used for military purposes, but must either be returned or kept safeguarded for civilian use. Development of domestic fuels or reactor designs may have been postponed because such international offers were commercially more attractive. It can thus be argued that such bilateral arrangements, as with the American AEC, are more effective in delaying nuclear weapons than is NPT.[8]

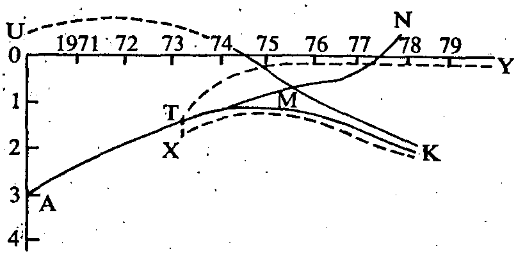

Figure 2. The impact of legal restraints.

Yet the combination of NPT plus nuclear assistance may be the most effective restraint of all. Nations having formally renounced nuclear weapons will be more willing to let their domestic independent nuclear options atrophy, to be replaced by external assistance. Nations which have been seduced into becoming dependent on outside assistance will be more willing to go ahead to sign NPT. The combination of the two restraining influences can be greater than the sum of their independent influences.

The presence of a treaty obligation thus forces us to discuss "legal" and "illegal" crash programs, distinctly defined by varying degrees of treaty violation. How long will it take an NPT-signatory nation in 1975 to manufacture bombs, if it seizes safeguarded stocks of fissionable materials (i.e., arresting and deporting, or simply ignoring, the IAEA inspectorate)? How long, if it instead openly and honestly revokes the treaty, giving the proper notices, surrendering or returning all safeguarded equipment and materials, beginning afresh to produce and accumulate plutonium free of any international legal commitment? Our "illlegal" curve (ATY) will show the physical capability of an NPT-signatory for cases in which it is prepared to ignore its treaty obligations completely. The "legal" (ATXK) curve conversely shows time-lags to the bomb for cases in which such treaty obligations are to be respected, that is, if no price in "international reputation for law-abiding behavior" is to be paid.

The distinction between "legal" and "illegal" crash programs is important, at least because it is another source of the semantic confusions under examination here. When asked how long it will take his country to make a bomb in 1975, some scientists will answer with reference to strict adherence to treaty obligations, others with total indifference to such obligations.

The ATN curve shows how capabilities might have developed if there had been no NPT. When compared to this "no-treaty" curve, signing NPT may thus have brought a nation closer to bombs for an interim period, at least via potential "illegal" bomb programs. Signing up probably has given a nation access to greater outside assistance in nuclear technology, and induced a greater foreign tolerance of big reactor projects or of plutonium-enrichment plants. Looking at the "legal" curve (ATXK) however, one sees that the bomb program time-lag has been increased. First, there is a flat addition of three months notification, at the date of signature. Second, the signatory state thereafter may be more likely to accept fuels and facilities under IAEA safeguards from other countries, rather than maintaining indigenously produced nuclear fuels and facilities, probably because these will be much more efficient and economical; hence the establishments legally free of bans on military use will actually begin to atrophy. The time-lag for "legal programs" will thus gradually expand again to level off at some number of years instead of months.

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[7] The reasoning on preemptive and surprise attack is spelled out quite fully in Thomas Schelling, *Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960), pp. 205–254.

[8] See Jan Prawitz, "A Nuclear Doctrine for Sweden," *Cooperation and Conflict*, 1968:3, pp. 184–193, for an account of the diverting of Swedish nuclear activities from military to purely civilian purposes, in part through the acceptance of foreign (American) assistance.

One must note that this latter "legal" ATXK curve will at best be a benchmark. It indicates what a state can do with roughly as much legal dishonor as would have been incurred in a bomb program before the treaty had been adhered to. In actuality, the benchmark may become unrepresentative in one direction or another. International morality might come to disapprove strongly of any state's exercising even its legal prerogative of withdrawing from the treaty; alternatively, international morality might come to tolerate states violating the treaty, when they were severely threatened, even without ever formally withdrawing from it.

Increasing Time-Lags

It will be interesting to examine the ways in which the curve could move away from zero, as the time-lag grew. Theoretically it is possible for a nation to lose its proficiency at physics, if scientists were to be driven out of the country or if research programs were allowed to wither for lack of funds (ATMK). Something like this happened in Germany between 1930 and 1935; if the Weimar Republic had survived, it could have had the atomic bomb earlier than Nazi Germany; as it happened, the German "crash program" curve moved away from zero during the years in which Jewish and other physicists were driven to leave the country.[9]

For another example, a nation that already has a nuclear stockpile and destroys it will have increased its "time lag" (UK). If all facilities capable of producing fissionable material were to be dismantled, this would widen such a gap even more.

Other possibilities exist, for there are some irreversible processes in the exploitation of economic opportunity. A nation might at some point have to choose between investing in a large power reactor project, or in a similarly extensive hydroelectric facility. Either of these would supply electricity, and would lower the market price of such electricity enough to make the other redundant. A military crash program that also produces needed electricity can be assigned a lower explicitly "military" price tag, and can be funded more rapidly and generously. Suppose that the hydroelectric project were built. If the electrical needs of a country thus had been satisfied by alternative means, the probable speed of any crash program would be reduced and the time-lag to weapons increased. All such pricing and costing thus depends on the environmental assumptions. Many nuclear electrical projects can conversely be rated as profitable if, and only if, the plutonium produced is explicitly assigned the higher market value of military purposes.

Even within the nuclear field, newer and more profitable technology may over time require less handling of fissionable materials, although the current trend is clearly in the opposite direction. As absolute weapons-capable facilities are thus closed down, to be replaced by newer non-weapons-capable plants, the time-lag to weapons would naturally grow again in countries not consciously pursuing weapons.

One might still vaguely cherish the hope that weapons-grade uranium or plutonium will never be required for any commercial purpose, that the fuels for power reactors can always be less pure. If this were so, the innocent processes of commercial development could indeed leave states still far from bomb capabilities. And if that were so, the Nuclear Non-Proliferation Treaty could indeed have been written still more tightly, to ban explodables as well as explosives, to preclude the possession of fissionable materials by any state outside the current five weapons states. Yet the exact definition of what is fissionable would require disclosure of relatively classified military information, and the facts here may never be settled for all time. For the U. S. or USSR to inform the world of the exact purity of uranium or plutonium required for a bomb might only accelerate the proliferation process. Research reactors may inevitably require weapons-grade materials in any event and so may certain power reactors some day.

A nation may similarly be induced by commercial consideration to depend on a foreign supply of enriched uranium or on foreign reprocessing of plutonium, thus again slowing and making commercially redundant any indigenous national program legally free to produce weapons. But here we are returning to the distinction between legal and illegal crash programs introduced above. In many circumstances, such a legal barrier would slow moves toward bombs just as much as physical obstacles would. In other cases, however, legal scruple will count for less. Even so "law-abiding" a country as Sweden might consider illegally seizing safeguarded plutonium or enriched uranium to make some bombs if a foreign army seemed about to cross its borders and advance on Stockholm. For the most dire circumstances, every advanced nation might thus become a short-time-lag nuclear weapons state, unless the physical technology of nuclear energy were to move countries away from this capability.

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[9] See Robert Jungk, *Brighter than a Thousand Suns* (New York: Harcourt, Brace and World, 1958) for a fascinating account of the distribution of nuclear expertise in the 1920s and 1930s.

It was earlier argued that the international inspector's explicit function will soon be made obsolete by the shrinking of the "time-lag to bombs." Perhaps the Non-Proliferation Treaty could thus have been better written without any provision for inspection, and so could the bilateral agreements by which nuclear-weapons states give technological and material assistance to nonweapons states. Inspection promises to pose many problems, in terms of the sheer cost of accommodating such international visitors, and the threats of commercial espionage or simple misunderstanding. Yet it would be an overstatement to say that international inspectors would serve no purpose any more once the crucial time-lag had fallen below six months. By their very presence, they may lengthen the physical time-lag.[10]

To be in with a relatively trivial point, no one can be certain that a bomb program would not still be detected somehow by the inspectors wandering around the premises. Since estimates of secrecy will have to be probabilistic, the addition of a safeguards system may yet deter a bomb program, out of fear that the outside world would be roused before the program could be completed. Even if they could no longer detect a bomb program any earlier than the cheating government wanted them to, the inspectors in position will be a continual and visible reminder of the state's treaty obligations, a reminder that some international retaliation will come after bombs are acquired, no matter how late it comes. Violating NPT will be a delicate decision for any state; if forgetting about international feelings were to make it easier for some state to decide to go ahead, the presence of inspectors will make these harder to forget.

The presence of inspectors generally upsets the legitimacy of purely national decisions on nuclear weaponry, in effect undermining the sovereignty of the nation in the nuclear field. It will be less easy for the crucial physicists to see a clear pattern of duty for themselves; some may become confused enough in their loyalties to betray a national bomb program to the IAEA representative passing through. Inspectors moreover make it more difficult for outside states to forget or back out of their threats of reprisal; the insult is compounded when one's nationals on duty as inspectors are ignored and defied.

Finally the IAEA safeguards procedures, however inadequate, force the government in question to institute national auditing and control systems which otherwise might not be established. With international inspection, any move to a bomb will at least require making an explicit national decision, rather than allowing the development to go forward by default. In states which otherwise might have let small cliques of scientists or military men launch military weapons programs, the inspectorate may thus forestall a bomb acquisition.

Exceeding the Crash Program

One must also reexamine the assumption that the curve cannot approach the zero-line at a more severe angle than 45 degrees (AZ). The time-lag at point A is defined only by conditions within the country in question. Naturally, every nation has a zero time-lag when another state already possessing nuclear weapons is prepared to give some away. The definition also excludes drastic redefinitions of the country in question; e.g., if the Common Market on short notice were to become sovereign, Luxembourg in that short time would have become (part of) a nuclear weapons state. The time-lag we will normally cite for Luxembourg assumes that there will be no merger with France and no gifts of nuclear warheads from the United States. But what if a group of French physicists tomorrow flee to exile in Luxembourg (perhaps bringing stolen fissionable material with them)? This may give the country a bomb by 1972, yet our curve for 1970, did not show a two-year time-lag. Indeed, to take a real-life example, could one have predicted in 1925 that the United States was only 20 years from the atomic bomb? With even J. Robert Oppenheimer studying in Germany, the balance of scientific talent might have suggested 1955 as

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[10] Some of the following argument was made earlier in a broader arms control context by Lawrence S. Finkelstein, "New Trends in International Affairs," *World Politics*, 18 (October, 1965), 117–126.

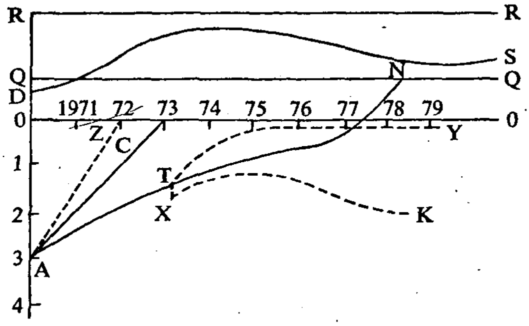

Figure 3. Redefinition of the baseline.

the bomb-acquisition date for the United States (and 1942 for Weimar Germany).[11]

The vertical axis on our graphing thus shows only the probable duration of a crash program utilizing the resources and technical manpower likely to exist in the country at any given time. It represents a prediction of how long military crash programs would take, and of how other programs will function in lieu of crash programs, given the current scientific potential available to particular national governments.

Redefinition of the Baseline

To add a semi-trivial category of conclusions to those already spelled out, we may differ as to what our zero time-lag line should represent. When we ask, "How far is Australia from the bomb?" do we mean only one Nagasaki-type plutonium bomb (probably not), or twenty such bombs, or one hundred; or do we mean hydrogen-bombs (if we do not find ordinary A-bombs particularly significant); or do we mean bombs plus "first-rate" delivery-systems? Scientists frequently give reassuringly long time estimates for some country's bomb program, and then afterwards explain that these estimates were meant to refer to a "sizable" stockpile, or to include extensive development of military bomber or missile systems.

Such longer estimates often will not be so reassuring; "two plutonium-bombs, delivered by an Israeli airliner" may not be insignificant. The variations here might be shown additively by using alternative baselines drawn parallel to each other (O-O, Q-Q, R-R) to distinguish various levels of accomplishment. By definition it takes longer to produce 50 bombs (R-R) than to produce 10 (Q-Q). Delivery systems, if they are relevant, may be more variable in their expansions of the time-lag. One could try to predict how available such systems will become "naturally" (i.e., with no explicit military intent) in the future. If a Boeing 707 were a ready-made bomber in 1961, perhaps SAM missiles reduce its effectiveness today. Conversely, if many nations decide to invest in space research, perhaps intermediate-range missiles will come to be close at hand for many of the countries which are about to acquire plutonium bombs. The zero baseline for "bombs-plus-delivery systems" might thus be a curved line (D-S), so that the time-lag for full military capability would be accurately shown.

Summary

The graphic formulation used here is intended to show the range of answers possible for the set of questions, "How far is India from the bomb?" The confusion and multiplicity of answers is of more than academic interest, for it shows that reassuring the outside world on one question in this range may not always be consistent with reassurance on another. Stemming the spread of nuclear weapons may not be hopeless, but it also may not ever be definitively accomplishable. There may be an important distinction between legal and illegal capabilities, once a treaty has been introduced, such that we will have to measure *political* capacities for violating treaties as carefully as *physical* capabilities. We will also have to consider the ranges of time-lags that bring a nation "too close" to a bomb, so close that opposing states would be tempted to try preemptive attacks, so that actual possession of bombs might have been preferable, so close that serious steps should be considered somehow to widen the time-lag.

Above all, the anticipation of nuclear proliferation may exhibit many of the same reiterative strategic calculation problems as does deterrence after the weapons are acquired. "How far is Japan from a bomb" may not be as important a question as "How far is Japan from being three months from a bomb"; the answer to the latter question may set some preemptive wheels whirring, in ways we wish to avoid.

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[11] This is of course a somewhat speculative extrapolation from the evidence offered by Jungk, *Brighter than a Thousand Suns.*

On the Concept of Ideology in Political Science*

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"Ideology" has been, and remains, one of the most persistent and ubiquitous concepts in modern political thought. No doubt this popularity is due to the absence in our political lexicon of alternative terms that link political thought and behavior in a satisfactory way. Yet, as one writer on the concept of ideology observes, "the growing popularity of the term has been matched, if anything, by its growing obscurity."[1] This ambiguity in the concept is reflected from author to author in a wide variety of definitions, explicit or implicit, and a lack of agreement regarding even the basic properties of ideology.[2]

If the concept of ideology is worth saving for political analysis—and the attempt seems warranted by both the concept's widespread use and the lack of an acceptable substitute—then the conceptual and terminological confusion surrounding it must be dispelled. The problem, I maintain, is not merely a formal one of defining ideology and using the definition uniformly, as if we had "the right to retreat into our own worlds of meaning, and demand only that each of us remain consistent within his own private terminology."[3] It seems necessary rather to inquire into the basis of the concept's strength and meaning. What are the theoretical and empirical grounds for using ideology in one way rather than another? How does this usage allow one to discriminate among phenomena and to call attention to salient features of what is being explained? Can the concept be formulated and defined in such a way that it is relevant to empirical inquiry? Only in light of the answers to such fundamental questions will a definition of ideology be comprehensible or persuasive. The aim of this paper, therefore, is to explore the theoretical bases for conceptualizing ideology in a particular way, and, in terms of this conceptualization, to offer a definition which, I believe, is relevant empirically. The discussion will proceed in three parts. Section one will examine the basic problem in the conceptualization of ideology, that of establishing boundaries of ideology vis-à-vis other cultural phenomena. This will lead, in the second section, to a presentation of criteria by which ideology may be distinguished from related cultural forms—especially myth and utopia—with which ideology is often confused. Section three will be devoted to an elaboration of the concept of ideology through an analysis of components suggested in section two, and will conclude with a definition of ideology based on these components.

Ideology and the Problem of Boundaries

The main impediment to developing an adequate conceptualization of ideology, I believe, has been the general failure of those using the concept to recognize the necessity for distinguishing ideology from the great backdrop of other cultural phenomena. In many conceptualizations, ideology is so all-inclusive that other symbolic forms are encompassed by it; because ideology is too comprehensive, it lacks "discriminating power"; and, lacking the power to discriminate among various, often similar, phenomena, the concept fails to achieve empirical relevance.[4] In other conceptualizations, this same problem of boundaries arises because ideology is subsumed under another and more comprehensive, symbolic form such as myth. In neither of these situations does the concept of ideology have much value for political analysis.

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[1] Giovanni Sartori, "Politics, Ideology, and Belief Systems," *American Political Science Review*, 63 (June, 1969), 398.

[2] The best review of the many meanings of ideology is provided by Arne Naess and Associates, *Democracy, Ideology and Objectivity: Studies in the Semantics and Cognitive Analysis of Ideological Controversy* (Oxford; Basil Blackwell, 1956), especially, pp. 141–176. An appreciation for the range of meanings, however, can be gained from the much shorter list collected by Robert E. Lane, "The Meanings of Ideology," in *Power, Participation and Ideology*, ed. Calvin J. Larson and Philo C. Wasburn (New York: David McKay, 1969), pp. 321–323.

[3] Hannah Arendt, *Between Past and Future: Six Exercises in Political Thought* (Cleveland: Meridian, 1963), p. 96.

[4] This difficulty is recognized clearly by Giovanni Sartori (pp. 398–400). Although I disagree with Sartori's concept of ideology, his work is important because it raises, in a self-conscious and systematic way, the problem of distinguishing ideology from other mental and cultural phenomena.

I have analyzed Sartori's approach in an article titled, "Sartori's Concept of Ideology: A Dissent and An Alternative," in *Public Opinion and Political Attitudes: A Reader*, ed. Allen R. Wilcox (New York: Wiley, forthcoming).

because its limits and differentia remain obscure. A brief exposition of some representative examples from the literature on ideology will illustrate the nature of the problem of boundaries and suggest how one might solve the problem satisfactorily.

Karl Marx has had perhaps the greatest continuing influence on the concept of ideology. Nevertheless, Marx was not the first to study ideology, nor did he invent the term. Francis Bacon may be said to have originated the study of ideology in his investigation of the preconceptions and prejudices, or "idols," which impede the exercise of scientific method.[5] As a distinctive term, however, ideology is the product of certain French post-Enlightenment theorists called the *Idéologues* who developed a theory of ideas based on sensationist philosophical principles similar to those of Locke.[6] The *Idéologues* regarded ideology quite simply as a "science of ideas." Ideology did not refer to a quality or type of thought, but to a technique for discovering truth and dissolving illusions. Marx, however, changed the meaning of ideology significantly, and, in doing so, cast it in its modern form.[7] Instead of being a method for discovering truth, in Marx's theory ideology designates a quality of thought—especially social thought—that is illusory or distorted. By using the concept of ideology in this way Marx raises for the first time the problem of limits. If ideology refers to a type or quality of thought, what are the boundaries of ideology and what are the relations of ideology to associated phenomena? Unfortunately, Marx's theory of ideology does not meet this problem satisfactorily, for he uses the term so comprehensively that it seems to include almost all cultural phenomena. As he views ideology, it encompasses all the mental misconceptions that constitute the failure of individuals to understand their alienated relationship to their surroundings and the significance of that relationship within the whole development of history.[8] Moreover, it is not clear in Marx's theory, to what degree these misconceptions are equal to consciousness itself.[9] Certainly, Marx maintains that in the stage of industrial capitalism and bourgeois society, the entire "cultural superstructure" is ideological, and within the superstructure he includes "the legal, political, religious, aesthetic [and] philosophic"[10] forms of ideology. In Marx's view, only Marxist social theory, based on a true understanding of history, appears free from ideological distortion.

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[5] An excellent account of Bacon's contribution to the theory of ideology is provided by Hans Barth, *Wahrheit und Ideologie,* 2nd ed., enlarged (Zurich: Eugen Rentsch Verlag, 1961), pp. 32–60.

[6] The most prominent of the *Idéologues* is generally considered to be Antoine Destutt de Tracy whose basic work on ideology was his *Elements d'Ideologie* (1801–1815). An excerpt from the *Elements d' Ideologie* in English translation, together with some comments on the work of the *Idéologues,* is contained in Richard H. Cox, ed., "The Original Concept of Ideology," in *Ideology, Politics, and Political Theory* (Belmont, California: Wadsworth, 1969), pp. 10–27. On the *Idéologues* see also H. B. Acton, "The Philosophy of Language in Revolutionary France," in *Studies in Philosophy,* ed. J. N. Findlay (London: Oxford University Press, 1966), pp. 143–167; Barth, pp. 13–31; George Lichtheim, "The Concept of Ideology," *History and Theory,* 4 (1965), 164–170; or, Jay W. Stein, "Beginnings of 'Ideology,'" *South Atlantic Quarterly,* 55 (April, 1956), 163–170.

After the Revolution of 1789, when France was still a democratic republic, the *Idéologues,* most of them members of the National Institute, sought to develop doctrines that would be a solvent for the *à priori* philosophical and theological speculations which had bolstered the old regime. The method the *Idéologues* developed to meet this double exigency was the "science of ideas" which they called "ideology," and through it, they aspired to provide a firm ground for reason by "reducing" all ideas to their genesis in sensation. Using this technique, the transcendent notions of religion and philosophy were subjected to criticism with the aim of hindering the perpetuation of false abstract principles which, they believed, impeded "not only the understanding of men amongst themselves, but also the building of the state and society." (Barth, pp. 16–17. Translation by Ilse M. Mullins.)

Their iconoclastic views naturally brought the *Idéologues* into conflict with Napoleon Bonaparte after his rise to the position of Emperor. He set out to discredit the science of ideas, which, it seemed, undermined political authority and promoted the dissolution of the body politic. Through his powers as head of state, Napoleon reorganized the Institute, abolishing the Second Class of Moral and Political Sciences, until then a sanctuary for the *Idéologues,* and he turned the terms "ideology" and "ideologue" into pejoratives, indicating that the *Idéologues* were visionary dreamers who had no understanding of practical political affairs.

[7] Of course, Marx's concept of ideology was influenced by antecedent developments in German philosophy, especially as found in the works of Hegel and Feuerbach. See, for example: Barth, pp. 61–96; A. James Gregor, *A Survey of Marxism: Problems in Philosophy and the Theory of History* (New York: Random House, 1965), pp. 3–37; Lichtheim, pp. 174–177; or John Plamenatz, *Ideology* (New York: Praeger, 1970), pp. 32–45.

[8] In relating Marx's concept of ideology, it is tempting to stop with his portrayal of ideology as a separation or "inversion" of thought and being, where ideas become reified and lose their relevance to one's existential sociological condition. The cure for these mental chimeras, therefore, would be "positive science." See Marx and Engels, *The German Ideology,* ed. R. Pascal (New York: International, 1947), pp. 13–15.

This, however, would be an incomplete account of ideology as Marx conceived it. That is because, for Marx, a true understanding of one's existential situation in a particular epoch necessitates not only "positive science," but requires, in addition, the capacity to understand the significance of that situation as measured from the perspective of "history as a whole." In the words of George Lichtheim: "To Marx, as to any Hegelian. the actual world of empirical perception was only an imperfect realization—at times indeed a caricature—of the real or rational world, in which man's essential nature (his rationality) will have overcome the reified existence he leads while the surrounding object-world is not perceived as the product of his own creativity. The attainment of this liberated state is the work of history, whose dialectic is not disclosed by empirical perception, but by critical (philosophical) reflection on the totality of the process" (p. 194).

The problem of boundaries in the conceptualization of ideology also arises in the work of Karl Mannheim, where the dominant meaning of ideology is "the whole outlook of a social group,"[11] its "total *Weltanschauung*,"[12] as conditioned sociologically by the group's political orientation, and temporally by its location in the ongoing historical process. Moreover, Mannheim universalizes ideology, insisting that even Marxism is an ideology.[13] To the extent that Mannheim does attempt to distinguish between ideology and utopia, the criteria of differentiation are of little value analytically. Ideologies, he says, are of two types. Those incorporating norms from the past and tending to preserve the status quo are called "ideologies." On the other hand, thought is called "utopian" if based on transcendent elements "which, when they pass over into conduct, tend to shatter, either partially or wholly, the order of things prevailing at the time."[14] A crucial problem with the distinction is that one cannot tell when thought is utopian except in the successful case where norms serve to shatter the present order, and only after they do.[15] Analysis must always be *post facto*.[16]

The inclination to use ideology as a comprehensive concept incorporating most of what we ordinarily view as the theoretical or systematic side of culture, and the corollary failure to distinguish ideology from other cultural forms, may also be seen in the works of more recent writers. In a book by L. H. Garstin, for example, the terms "ideology," "myth," "creed," "conceptual scheme," and "*Weltanschauung*" are used pretty much interchangeably.[17] Garstin says that "all societies are characterized by *weltanschauungs* [sic] of one kind or another."[18] As basic presuppositions, these *Weltanschauungen* or ideologies function to integrate beliefs and ideals and to give life meaning. In this sense ideologies exist equally in the primitive cultures of Malinowski's Trobriand Islanders or Ruth Benedict's Kwakiutls, in the premodern civilization of medieval Christianity, and in modern secular society.[19] Another author, Harold Walsby, even more categorically states that ideology refers to the inchoate underlying presuppositions of a society's world view.[20] Ideology, he argues, is not limited to

> basic ideas underlying some system of economic or political theories. [Ideology includes] the basic ideas (or rather, the assumptions) underlying *any* system of ideas. And since we shall find that all ideas (or more definitely, all propositions) are based on, or take for granted, certain cognitive assumptions, and that, moreover, on the basis of these assumptions, propositions can be classified or divided into different groups or systems, then it can be seen that our extended definition will cover the whole field of propositions—political, economic, religious, philosophical, scientific or otherwise.[21]

The literature of the so-called "end-of-ideology" school which flourished in the 1950s and early 1960s[22] also disregards the problem of distinguishing between ideology and other cultural forms. After a perusal of their writings, one critic finds that the members of this movement tend to share a view of ideologies as " 'absolute utopias.' "[23] One may also perceive in this literature a propensity to use the term "ideology" coextensively with the phrase "secular religion."[24] In either case, the contours of ideology are nebulous and of little analytical merit.

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[9] Compare, for example, Marx and Engels, pp. 13–15, with Marx, "Excerpt from *A Contribution to the Critique of Political Economy*," in *Marx and Engels: Basic Writings in Politics and Philosophy*, ed. Lewis S. Feuer (Garden City, New York: Doubleday, 1959), p. 44.

[10] Marx, "Excerpt from *A Contribution* . . . ," p. 44. In Marx's view, myth and utopia, as types of distorted theoretical thinking, are also forms of ideology. On myth, see Marx, "Excerpts from *The Eighteenth Brumaire of Louis Bonaparte*," in *Marx and Engels: Basic Writings* . . . , ed. Feuer, especially pp. 320–323. See also Harold Rosenberg's excellent analysis of the *Brumaire* in his "The Resurrected Romans," *The Kenyon Review*, 10 (Autumn, 1948), 602–620. On utopia, see Marx and Engels, "*Manifesto of the Communist Party*" and *Engels*, "*Socialism: Utopian and Scientific*," in *Marx and Engels: Basic Writings* . . . , ed. Feuer, pp. 37–39, 68–111, respectively.

[11] *Ideology and Utopia*, trans. Louis Wirth and Edward Shils (New York: Harvest, 1936), p. 59.

[12] Mannheim, p. 57.

[13] Mannheim, pp. 125, 277.

[14] Mannheim, pp. 192–193.

[15] Mannheim argues that "wish-images" are not utopian until they pass over into "actual conduct" (p. 193).

[16] For a discussion of this difficulty in Mannheim's thought, See Helmut R. Wagner, "Mannheim's Historicism," *Social Research*, 19 (Spring, 1952), 320–321.

[17] *Each Age is a Dream: A Study in Ideologies* (New York: Bouregy and Carl, 1954). Compare pp. 6, 7, 14, 27, 32, 34, 35, 37, 38.

[18] Garstin, p. 27.

[19] Garstin, pp. 10–47.

[20] *The Domain of Ideologies: A Study of the Origin, Development and Structure of Ideologies* (Glasgow: William MacLellan, 1947).

[21] Walsby, p. 142 (author's emphasis).

[22] See Raymond Aron, *The Opium of the Intellectuals* (New York: W. W. Norton, 1962); Daniel Bell, *The End of Ideology: On the Exhaustion of Political*

Finally, it will be instructive to examine an approach which offers a relatively more sophisticated—but, it will be argued, an unsuccessful—attempt to distinguish among ideology, myth and utopia. In the schema of Harold D. Lasswell and Abraham Kaplan, "the *political myth* is the pattern of the basic political symbols current in a society."[25] Myths are described as "certain fundamental assumptions which at the time, whether they be actually true or false, are believed by the mass of the world to be true with such confidence that they hardly appear to bear the character of assumptions."[26] In Lasswell and Kaplan's view, myth comprehends ideology and utopia because *"ideology* is the political myth functioning to preserve the social structure; the *utopia,* to supplant it."[27]

The approach of Lasswell and Kaplan makes no attempt to analyze the *structure* or form of ideology or to note the ways in which it differs structurally from other symbolic forms. The various types of political myth, including ideology and utopia, are distinguished on a *functional* basis. Following Mannheim's example, therefore, Lasswell and Kaplan argue that:

> [A] utopia as here defined need not be "utopian" in the sense of either impracticability or perfection. We call a pattern of political symbols a utopia if their function in the political process is to induce fundamental changes in power relationships or practices; an ideology, if they serve to maintain the given power patterns. . . .
>
> The definitions are based on the way the symbols function, not on characteristics of the symbols themselves. Symbols functioning at one time as utopias may at another serve as ideology, as indeed usually happens in the case of successful revolution—utopian symbols are retained regardless of their increasing divergence from the power facts.[28]

This functional approach to differentiating between ideology and utopia serves to remind us that the same symbolic structure can have differential functions vis-à-vis the political system. This was the case with liberalism and Bolshevism, for example, each of which played a revolutionary role at one historical juncture and a preservationist role at another. For a number of reasons, however, Lasswell and Kaplan's approach does not solve the problem of boundaries in the conceptualization of myth, ideology, and utopia.

First, their conception of utopia is subject to the criticism made earlier of Mannheim; there is no way to know whether thought is utopian except in the positive instance where it facilitates the breakdown of existing power relationships or practices, and only after the fact. In other than a *post facto* way, therefore, the conception is analytically worthless.

In a second sense, moreover, this approach

Ideas in the Fifties (New York: The Free Press, 1965); Albert Camus, *The Rebel: An Essay on Man in Revolt* (New York: Vintage, 1956); Seymour Martin Lipset, *Political Man: The Social Bases of Politics* (Garden City, New York: Doubleday, 1960); Edward Shils, "Ideology and Civility: On the Politics of the Intellectual," *Sewanee Review,* 66 (Summer, 1958), 450–480, and Shils, "Letter From Milan: The End of Ideology?" *Encounter,* No. 26 (1955), 52–58.

Much of the literature of this movement and its critics is found in Chaim I. Waxman, ed. *The End of Ideology Debate* (New York: Funk and Wagnalls, 1968).

[23] William Delany, "The Role of Ideology: A Summation," in *The End of Ideology Debate,* p. 295.

[24] See, for example, Aron, pp. 265–294; Bell, pp. 400–401; Camus, pp. 110–132; and Shils, "Ideology and Civility . . . ," pp. 457–466.

[25] *Power and Society: A Framework for Political Inquiry* (New Haven: Yale University Press, 1950), p. 116 (author's emphasis). For an analysis of political symbols similar to that presented in *Power and Society,* see also, Lasswell, "The Language of Power," in *Language of Politics: Studies in Quantitative Semantics* by Lasswell, Nathan Leites and Associates (New York: George W. Stewart, 1949), pp. 3–19.

[26] Lasswell and Kaplan, p. 117, quoting A. V. Dicey. Lasswell and Kaplan note the similarity between their " 'myth' " and, among others, "Marx's 'ideology,' " and "Mannheim's 'ideology' and 'utopia' " (p. 117).

[27] Lasswell and Kaplan, p. 123 (author's emphasis). In a footnote on this page, it is suggested that the term "utopia" is retained because of its "familiarity in technical contexts," but that the term *"countermyth"* might be "less misleading" (author's emphasis).

In this section Lasswell and Kaplan also conceptualize other aspects of the political myth, each of which has a distinct political function. These are: the *"political doctrine"* which "formulates basic expectations and demands;" the *"miranda"* which consists "of basic symbols of sentiment and identification;" and, the *"political formula"* which "is the part of the political myth describing and prescribing in detail the social structure" (pp. 117–123; 125; 126–133, author's emphasis).

I shall concentrate on the relationships between political myth, ideology and utopia, because they relate more directly to the general theme of this paper. Some of the remarks that follow, however, would apply, *ceteris paribus,* to the logical relationships between political myth, doctrine, miranda and formula, as well.

[28] Lasswell and Kaplan, p. 123.

has only limited value, considered taxonomically. Using Lasswell and Kaplan's distinctions, we are put in the logically odd position of saying, for instance, that a particular symbol system was utopian, is now ideological, and may be utopian again, all without any significant change in the symbol system itself. And, at the same time that the system is either ideological or utopian, it remains mythical.

Third, to confuse the problem of classification even further, the same criteria of differentiation are not applied to all the symbolic structures under consideration in a way that would provide common units of comparison. On the one hand, the criteria for myth are more or less structural—involving fundamental assumptions about reality so basic that they do not appear to be assumptions at all. On the other hand, the criteria for ideology and utopia are functional—involving their propensity either to maintain or to alter existing political practices.

Fourth, the differentia proposed by Lasswell and Kaplan fail to recognize established usages or to honor distinctions which scholars normally utilize. For example, are the detailed schemes of social perfection portrayed in Plato's *Republic* and More's *Utopia* to be considered nonutopian because they never served "to induce fundamental changes in power relationships or practices"? On the other hand, does it make sense, at least without considerable qualification, to call Marxism utopian? Marx and Engels not only criticized "utopian" socialists for painting "fantastic pictures of future society" and for rejecting "all political, and especially all revolutionary, action"[29] but they also refused to spell out the details of communist society for fear of being considered utopian.[30]

Finally, Lasswell and Kaplan's approach is weak because it relies on an *a priori* definition which limits the empirical relevance of their conceptions. Instead of identifying ideology and utopia as cultural forms and then seeking to ascertain their functions in the political system, Lasswell and Kaplan define ideology and utopia in terms of their functions. The result is to make it impossible through empirical inquiry to falsify the assumption that ideology is functional, or that utopia is dysfunctional, to the maintenance of the political system. If, for example, Marxist "ideology" could be shown to be dysfunctional to the system, the notion that it is nevertheless functional could be saved by noting that, in this case, Marxism is functioning as utopia. But, surely, as an empirical matter, we want to know just what consequences certain standardized cultural forms have for the system. The consequences may be functional, dysfunctional or neutral. And this assessment can be made only by identifying the patterns, delineating the system, and studying their relationship empirically, not by settling the outcome through definitional fiat.[31]

A Comparison of Ideology, Myth, and Utopia

It was argued, in the preceding section, that the problem of boundaries in the conceptualization of ideology arises from the failure to maintain distinctions between ideology and other cultural phenomena (especially myth and utopia) with which ideology is often confused. Implicit in this argument was the additional notion that only by discerning differences in the cultural systems or symbolic structures themselves will one be likely to arrive at differentia which solve the problem of boundaries and afford empirically relevant concepts. The possibilities of this approach have been largely ignored by writers on ideology. There are, however, a few significant exceptions to this general rule, and to these writers I am considerably indebted. Lee C. McDonald recognizes that systematic distinctions between myth and ideology are rarely maintained,[32] and he offers some valuable suggestions on how this might be done.[33] Another writer, Ben Halpern, has made a most sophisticated and helpful analysis of ideology and myth as cultural forms in conjunction with a consideration of their sociologically dynamic properties.[34] Some additional insights into differences between ideology and myth as cultural forms are found in a short essay by Kenneth Burke.[35] Clifford Geertz has written what is, in my view, the best account of ideology as a cultural system, especially as it contrasts with the system of science.[36] Finally, Carl J. Friedrich has some useful ideas on the differences between myth, utopia, and ideology as cultural phenomena, although his analysis is dominated by a concern with the functional status of these forms.[37]

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[29] Marx and Engels, *"Manifesto . . . ,"* p. 38.

[30] Marx and Engels' well-known reference to communist society in *The German Ideology*, hardly counts as an example of detailed utopian planning: "[I]n communist society, where nobody has one exclusive sphere of activity but each can become accomplished in any branch he wishes, society regulates the general production and thus makes it possible for me to do one thing to-day and another to-morrow, to hunt in the morning, fish in the afternoon, rear cattle in the evening, criticize after dinner, just as I have a mind, without ever becoming hunter, fisherman, shepherd or critic" (p. 22).

The most "utopian" aspect of Marxism is perhaps the expectation that in communist society something resembling utopian planning would be possible because, there, production is carried out "by freely associated men, and is consciously regulated by them in accordance with a settled plan." Karl Marx, *Capital*, Vol. I (Moscow: Progress, 1965), p. 80.

[31] This argument is similar to one made by Robert Merton against the practice of assuming that every structure has a function. Whether this is so can only be decided on the basis of empirical inquiry. See, *Social Theory and Social Structure*, 2nd ed., rev. and enlarged (Glencoe, Illinois: The Free Press, 1957), pp. 32–36.

[32] "Myth, Politics and Political Science," *Western Political Quarterly*, 22 (March, 1969), 150.

[33] McDonald, p. 149 and, especially, "Politics and Myth," in John A. Hutchison and Lee C. McDonald, *Myth, Religion and Politics* (The Methodist Church: Division of Higher Education, General Board of Education, 1965), pp. 26–27.

I will attempt to incorporate and extend the insights of these writers by developing a typology that distinguishes among all three of the forms—ideology, myth, and utopia—and which does so with primary attention to their intrinsic features. Several prefatory remarks, however, are in order. First, there is no intention of presenting more than a general schema outlining the main factors which differentiate myth, utopia, and ideology. Second, therefore, the analysis concentrates on the salient differences among these cultural phenomena, rather than on the complexity of interrelationships among them. It should be remembered that, empirically, ideologies often include elements characteristic of myth, such as the notion of the social contract in liberalism, or the importance of the founders in Marxism-Leninism. And in most ideologies there is also an ideal of perfection, often unarticulated, which one might describe as "utopian." In an attempt to portray the general features of myth, utopia, and ideology, however, it will simplify matters to concentrate on prototypes. Moreover, inasmuch as these types are comprehensible it will be easier to discern the mythical and utopian elements incorporated by particular ideologies.

The discussion will begin with an analysis of the historical perspective implicit in ideological thought, and that, in turn, may be facilitated by placing ideology in historical perspective. Ideology, I maintain, is largely a modern phenomenon. It has little in common with a traditional social outlook, and although it can be discerned in earlier political movements,[38] it is not ubiquitous in Western thought before the French Revolution. The rise of ideology, in fact, appears to correspond to the breakdown in the traditional perspectives which characterized the *Ancien Régime.*

In times of historical crisis, when fundamental, time-honored, "self-evident" truths are questioned, ideologies bring the issue of a society's worth to the level of consciousness and guide men's actions over historical terrain that is, looked at in comparison to traditional society, terrifyingly unfamiliar and uncharted. This, I believe, helps to explain why ideologies are rapidly spreading from the West to the societies of the so-called "developing" world, where traditional order is being disrupted and transformed. Recognition of the intimate relationship between rapid historical change and the appearance of ideologies, however, does not mean that ideology is a dependent variable—a mere mental reflex generated by the personal, structural and cultural dislocations in developing societies. Inasmuch as ideology conceptualizes the historical process and orients human beings for shaping it, ideology is itself an active agent of historical change.

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[34] "The Dynamic Elements of Culture," *Ethics,* 65 (July, 1955), 235–249. Although Halpern is concerned with the socially dynamic functions of ideology and myth, he first establishes criteria for these phenomena, the functions of which are studied. He is attempting, in fact, to correct a deficiency in Talcott Parsons's theory of action which, while it studies the functions of cultural phenomena vis à vis the social system, "says very little about the systematic character of culture itself," or about how cultural forms guide actors in the social system (p. 236).

[35] "Ideology and Myth," *Accent,* 7 (Summer, 1947), 195–205, especially 198–200.

[36] "Ideology as a Cultural System" in *Ideology and Discontent,* ed. David E. Apter (New York: The Free Press, 1964), pp. 47–76.

[37] *Man and His Government: An Empirical Theory of Politics* (New York: McGraw-Hill, 1963), especially pp. 83–93 and pp. 94–105.

My view is that the question of an ideology's functions for a political system may be extremely important to the scientific understanding of political phenomena, and it is always, therefore, a matter with which students of ideology might be legitimately concerned. Two caveats, however, are in order. First, as I indicated in the previous section, questions about functions are empirically useful only after the identification of both what is functioning, and the system in relation to which there is a function. Second, the search for functions, with its emphasis on systemic patterns, should not obscure the fact that the patterns are abstractions from the concrete activities of individual actors who have feelings, motives, and purposes which are expressed and structured by the symbolic forms utilized by the actors. Friedrich is not susceptible to criticism regarding this second factor, although, regarding the first, he has a propensity to define ideology in terms of its functions.

The analysis of ideology in this paper, while relevant to problems of system-maintenance, is more directly concerned with how individuals and groups of individuals utilize symbol constructs to comprehend reality and direct their activities. For statements regarding the importance of not neglecting this relationship between individuals and culture in social explanation, see: Geertz, pp. 52–65; and, Halpern, pp. 235–236.

[38] Some find the beginnings of ideology in earlier millennial Christian movements. See, for example, Melvin J. Lasky, "The Metaphysics of Doomsday," *Encounter,* 32 (January, 1969), 36–47; or, Michael Walzer, *The Revolution of the Saints: A Study in the Origins of Radical Politics* (Cambridge, Mass.: Harvard University Press, 1965). A more compact version of Walzer's view may be found in his article, "Puritanism as a Revolutionary Ideology," *History and Theory,* 3 (1963), 59–90.

To Michael Oakeshott, the watershed of ideological politics appears to be the American, not the French, Revolution. See his "Rationalism in Politics," in *Rationalism in Politics and Other Essays,* by Oakeshott (London: Methuen, 1962), p. 28.

By viewing ideology as it relates to the emergence of modern society, it is possible to discern what I consider to be a crucial aspect of ideological thought, and a basic criterion for differentiating between ideology, myth, and utopia. Ideology incorporates a type of "historical consciousness" characteristic of the modern age, in which a high degree of social change is recognized, where " 'change' [means] the coming into being of new and specifically *different* constellations and not merely the replacement of one set of conditions by a new set of the same specific type, or simply the biological features associated with ageing in any natural process."[39]

Since it perceives discontinuities in human society and culture, historical consciousness is concerned about the things to come. Hence, there is a need to invent conceptions of the future. How the future is conceived will be greatly influenced, of course, by how the present and the past are understood. But while there may be some sense of tradition in historical consciousness, tradition is no longer accepted as a sufficient guide to human action in a historical context. In historical consciousness —and in ideology—the shape of the future, the nature of historical change, and the limits and possibilities of human control over these changes, become questions of overwhelming importance.[40]

Historical consciousness, therefore, implies the possibility of imagining qualitatively new social arrangements, of empathetically placing oneself in them, and of conceptualizing the extent to which individuals, including oneself, might be causally effective in bringing new arrangements about. This, it seems, is not possible in the "traditional consciousness" of those who inhabit traditional societies. For example, David Riesman observes that in a series of interviews conducted in the Middle East, a Turkish peasant, when asked what he would do if he were president, responded:

> 'My God! How can you ask such a thing? How can I . . . I cannot . . . president of Turkey . . . master of the whole world?'
>
> Correspondingly, [Riesman continues] many of the tradition-minded in these interviews, asked where they would like to live if they could not live in their native villages, said they would rather die; they could not conceive of living anywhere else, any more than of being somebody else.[41]

Clearly, the peasant views himself as being politically impotent, the member of a rigidly defined social structure which has always been, and will continue to be, essentially as it is. For him, there can be no program of political action; unable to conceive of a new social order, he would find it quite unthinkable to make any effort on behalf of its emergence. He is simply not historically aware.

The modern historical time which is an essential part of ideology differs fundamentally from the nonhistorical time of myth or utopia, both of which find their classic expression in the pre-modern era. In historical time, events are conceived of as occurring once at some determinate point on a linear continuum. Historical events, therefore, are viewed as unique, and social activities or structures, understood as historical events, are *sui generis*. In mythical time, on the other hand, the concern is not with what is unique in human experience, but with what is universal; "not with what happened but with what happens . . . in other words [with] the typical or recurring element in action."[42] In the mythical time of primitive man, Eliade reports, "events were not thought of as happening once for all (as, we would say, true events must do) but were 'happening' eternally. The impression of an inexorable forward movement in time is an illusion; there is only 'the eternal return,' the constantly repeated cycle, the endless round."[43] Noting this emphasis in myth on the recurring aspects of human action, Halpern says:

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[39] Frederick M. Barnard and Jene M. Porter, *Discontent and Political Development: A Discussion of Some Contemporary Methodological and Substantive Problems* (mimeographed), paper delivered to the Canadian Political Science Association meeting, Calgary (June 5, 1968), p. 12.

[40] Although there are enormous differences in their theoretical content and conclusions, most variants of liberalism, socialism and fascism view politics as working in accord with historical developments toward more preferable social arrangements. As an ideology, conservatism also considers the relationship between historical developments and political action, but it argues that given the nature of history and the limits of politics, men should avoid any but the most circumscribed political and social innovations. A basic difference between ideological conservatism and a traditional view, incidentally, is that, in the former, historical change is a practical and intellectual problem, whereas, in the latter, no such problem arises.

[41] "Introduction," in *The Passing of Traditional Society: Modernizing the Middle East,* by Daniel Lerner (New York: The Free Press, 1964), p. 3.

[42] Northrop Frye, "New Directions From Old," in *Myth and Mythmaking,* ed. Henry A. Murray (Boston: Beacon, 1968), p. 116. Frye uses these phrases to describe the subject matter of the poet, but as he recognizes, the description applies equally well to the content of myth.

Recognizing the intimate relationship between poetry and myth, McDonald stresses the public nature of the latter: "Myths are poetry, but a special kind of poetry —the poetry men live by" ("Myth, Politics and Political Science," p. 141).

> In fact, the process involved in a myth vision is essentially a poetic one: it requires discovering in superficially diverse act-complexes underlying morphological identities—so that Proserpine descending to Hades exemplifies all experiences of temporal mortality and rebirth, the revolving seasons, the succession of day and night, and the cycles of sowing and reaping.[44]

In politics, some of the most important myths concern the founding of the polity.[45] The founding myth often refers to heroic figures, real or imaginary, who created the political order. When persons invoke the name or recount the exploits of the founders, past and present are merged; the past is renewed in the present, even as the present draws significance from the past. By such practices, political communities celebrate the essential identity between the original act of founding and the continuing actions that extend and preserve the polity.[46] Hence in a society characterized by mythical consciousness, the social structure is not viewed as an arrangement that might be altered by each generation according to its perceptions of justice or convenience, but as a sacred and timeless order which is sanctified by the myths that explain its importance and its origin.[47]

Utopia, like myth, conceptualizes time as recurrent and nonhistorical. First, because utopia presents an intellectually reconstructed world without sin or human frailty,[48] and because of its emphasis on a perfected and closed social structure, it "does not, and perhaps cannot, recognize the unending flow of the historical process."[49] Although there is change in utopia, it is not historical change: "all processes going on in utopian societies follow recurrent patterns and occur within, and as part of, the design of the whole."[50] In utopian society there is no *conception of a historical process within which* social transformation is possible. "Considered as a final or definitive social ideal, the utopia is a static society; and most utopias have built-in *safeguards against radical alteration of the* structure."[51] There is also a second sense in which classical utopia poses a disjunction with history. It is ahistorical because it neither incorporates an expectation that the ideal will ever be realized nor presents any historical program for its actualization. In the words of Judith Shklar, utopia portrays "a timeless 'ought' that never 'is.' "[52] In his utopia, therefore, Thomas More echoes Plato's pessimism regarding the possibility of bringing utopia into existence through political means.[53] The function of classical utopia is that of criticism; by postulating a condition of perfection, it reveals the limits of actual society. According to Leo Strauss, this is the role performed by Plato's utopia: "Socrates makes clear in the *Republic* of what character the city would have to be in order to satisfy the highest need of man. By letting us see that the city constructed in accordance with this requirement is not possible, he lets us see the essential limits, the nature, of the city."[54] But although the perfection of utopia stands as a condemnation of the real political system, the utopian model is not a guide for the existing system's transformation. As an object of contemplation it is "'prolonged and enjoyed for its own sake,'"[55] but it is not, in fact, a guide to any actual doing.

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[43] Quoted by John Knox, *Myth and Truth* (Charlottesville: The University of Virginia Press, 1964), p. 56.

[44] Halpern, p. 238.

[45] On the myth of the founding, see Friedrich, p. 96 and pp. 393–396.

[46] Regarding the mythical renewal of the past in the present, Thomas Mann says: "[L]ife in the myth, life, so to speak, *in quotation*, is a kind of celebration, in that it is a making present of the past, it becomes a religious act, the performance by a celebrant of a prescribed procedure; it becomes a feast. For a feast is an anniversary, a renewal of the past in the present. Every Christmas the world-saving Babe is born again on earth, to suffer, to die, and to arise. The feast is the abrogation of time, an event, a solemn narrative being played out conformably to an immemorial pattern; the events in it take place not for the first time, but *ceremonially according to the prototype*." See "Freud and the Future," in *Myth and Mythmaking*, ed. Murray, p. 374.

[47] See Bronislaw Malinowski, *Magic, Science and Religion and Other Essays* (Garden City, New York: Doubleday Anchor, 1954), pp. 100–101.

[48] Myth has a religious quality of high seriousness; it is believed to be true and it deals with the intense, even tragic, character of real human experience. See Susanne K. Langer, *Philosophy in a New Key: A Study in the Symbolism of Reason, Rite and Art* (New York: Mentor, 1951), pp. 151–153.

Ideology is also characterized by serious intent; it is oriented to the problems of human beings living in society, and it is concerned with whether and how the human social condition may be improved. Utopia, on the other hand, is frankly imaginary and lacks the drama or tension of myth and ideology.

[49] Ralf Dahrendorf, "Out of Utopia: Toward a Reorientation of Sociological Analysis," *The American Journal of Sociology*, 64 (September, 1958), 115.

[50] Dahrendorf, p. 117.

[51] Northrop Frye, "Varieties of Literary Utopias," *Daedalus*, (Spring, 1965), p. 329. See also, Judith Shklar "The Political Theory of Utopia: From Melancholy to Nostalgia," in this same issue. Utopia, she says, "is an expression of the craftman's desire for perfection and permanence. That is why utopia . . . is of necessity a changeless harmonious whole, in which a shared *recognition* of *truth unites* all *the* citizens. Truth is single and only error is multiple. In utopia there cannot, by definition, be any room for eccentricity" (p. 371).

[52] Shklar, p. 372.

[53] See Thomas More, *Utopia*, trans. H. V. S. Ogden (New York: Appleton Century-Crofts, 1949), pp. 18 and 24. In his closing sentence More admits "that there are many things in the Utopian Commonwealth that I wish rather than expect to see followed among our citizens."

In *The Republic*, see especially Book VI. For the argument that, according to *The Republic*, it is impossible to bring the good city into being, see the discussion by Leo Strauss, *The City and Man* (Chicago: Rand McNally, 1964), pp. 118–138.

Judith Shklar has argued that after the French Revolution, utopias (in the classical sense) decline in importance. To the extent that utopias continue as a genre, they tend to retain the outward form, but lack the essence of, the classical archetype. Influenced by the "future-directed activism" of the postrevolutionary age, these utopias enter the stream of history as possible future societies, although their proponents argue only for their "historical non-impossibility."[56] In the words of Bertrand De Jouvenal, the typically utopian "journey to a distant place has been gradually superseded by the projection into a time distant in the future. . . ."[57]

In this sense, these utopias resemble ideologies more than did their classical predecessors. But there is still a significant difference between ideologies on the one hand, and modern "historical" utopias on the other. The latter enter the realm of history in the sense that they might happen, but they do not confront the historical problems associated with transforming existing society. There is no preoccupation as in ideology with how, given the sociological context, the limits and possibilities of human action, and the political resources available, the good society might be more nearly approximated—or, to put it negatively, how the bad society might be avoided. And, while ideologies often contain "utopian" ideals of justice or the good society, they are not expressed as elaborate and aesthetically balanced models of perfection suspended in a "no place" or a "some time." In ideology such ideals are linked to concrete programs and strategies having immediate social relevance. As such they become motives for action rather than mere intellectual constructs. No doubt this helps to explain why ideologies, not modern utopias, are associated with political parties and their programs.

In addition to conceptualizing change differently, myth, utopia and ideology may also be distinguished according to the symbols[58] by which they are constituted. Since it utilizes the highly figurative and metaphorical language of poetry, of course myth is not logical in form. Indeed, myth may be viewed as a mode of expressing basic principles or fundamental truths without recourse to philosophical language.

> [S]uppose that you had no such words as "substance," "essence," "principle," "logical," as used in our traditional philosophical idiom. Yet at the same time you did feel the need to justify the social practices and relations of your society. . . . Is there any spontaneous vocabulary that might substitute for the missing terms?
>
> There is indeed, and in primitive "myth" we see it everywhere. "Principles" means "firsts." The corresponding word in Greek is the word from which we derive "archetype," "archeology," "archaic," and the like. Hence, if you want to deal with logical principles, or "firsts," and don't have such a language, you get the equivalent by talking of *mythical* firsts. To derive a culture from a certain mythic ancestry, or ideal mythic type, is a way of stating that culture's essence in narrative terms.[59]

The language of ideology is replete with metaphor; nevertheless, ideology, unlike myth, is cast in the form of logic. Whereas myth finds expression in the apposite image, ideology relies also on the correct category. Whereas myth personalizes events, encouraging "personal identification" with its symbols,[60] ideology is convincing, in part, because it facilitates a logical grasp of events and situations. Whereas myth is essentially plastic and spontaneous, something that grows with the telling, ideology can be written down and its truths "worked out" logically.[61] Whereas the images of myth compress meaning, ideology tends to explicate the significance of events, situations and possible courses of human action.

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[54] Strauss, p. 138.

[55] Shklar, p. 371.

[56] Shklar, pp. 374–378.

[57] "Utopia For Practical Purposes," *Daedalus* (Spring, 1965), p. 438.

[58] I use the term "symbol" in the broad and untechnical sense that "[a]ll linguistic expression is symbolic representation." (Friedrich, p. 100).

[59] Burke, pp. 199–200 (author's emphasis). Burke suggests that ideology, with its philosophical vocabulary, is a way of translating mythic truths into the forms of discursive thought (p. 201). See also, Halpern, pp. 240–241.

[60] Halpern, p. 240.

[61] Hence ideology, unlike myth, can be identified as the mental product of a specified political or cultural elite.

In comparison with myth, ideology expands meaning, but in relationship to the language of utopia, the language of ideology is decidedly more sparse and economical. Utopia, being an imagined condition of social perfection, tends toward elaborate description, often down to the most minute detail. Ideology, on the other hand, informs political action. Hence, far more than utopia, it is inclined to simplify alternatives, to reduce complex situations to understandable proportions so that action is possible. Whereas utopia expands the details, ideology appraises them, selecting some as especially relevant, and relegating the rest to the background.

The Components of Ideology: An Elaboration

The foregoing comparative treatment of ideology, myth, and utopia focuses on a fundamental factor in ideology—historical consciousness. This factor, in turn, suggests other distinguishing features of ideology, although in the preceding section these latter differentia were either developed only briefly or merely left implicit in the analysis. In this section, therefore, the remaining criteria of ideology will be explicated and discussed in detail. These are, in the order of their consideration: (1) cognitive power; (2) evaluative power; (3) action-orientation; and, (4) logical coherence.[62]

The cognitive power of ideology is attested to by Philip Converse in his analysis of belief systems in mass publics.[63] In the group surveyed, he finds a high positive correlation between the level of their knowledge about politics, the degree of their political activism, and the amount of "constraint" in their political beliefs. The notion of constraint is extremely important regarding the cognitive power of ideology, and it is also central to the notion of logical coherence, a feature of ideology to be discussed later. In Converse's usage, *constraint* refers to the logical coherence or consistency between various components of a respondent's beliefs, and he defines it as "the success we would have in predicting, given initial knowledge that an individual holds a specified attitude, that he holds certain further ideas and attitudes."[64] Constraint, therefore, refers not to randomly related specific attitudes but to attitudes which are organized into "wide ranging belief systems."[65] By emphasizing the variable of constraint in his measurement of political ideologies, Converse accentuates the important cognitive role they play in arranging political information so that it is meaningful to "the truly involved citizen" who seeks to understand, and to participate in, political life.[66] Converse asserts that for these people, highly constrained ideologies are able to organize "economically" extensive information which contains many elements into systems whereby the elements "may be described and understood."[67] He maintains that

> the use of such basic dimensions of judgment as the liberal-conservative continuum betokens a contextual grasp of politics that permits a wide range of more specific idea-elements to be organized into more tightly constrained wholes. . . . With it, for example, new political events have more meaning, retention of political information from the past is far more adequate, and political behavior increasingly approximates that of sophisticated "rational" models, which assume relatively full information.[68]

Hence, Converse says of the ideologue and near-ideologue: "Empirically they differ sharply from the less sophisticated in their attention to new political events and in the size of their store of information about past events."[69]

At first glance, it may seem odd that the cognition and retention of information about politics should be promoted through the use of abstract categories like "liberal" or "conservative," or that Converse would suggest an affinity between ideologies and "rational" models. Rationalistic abstractions, however, are vital to various forms of human understanding. Abraham Kaplan observes that even in the primordial, essentially spontaneous, cognitive activity of "identifying" and symbolizing recurrent aspects of experience, abstraction and generalization occur. "There is," he says, "no cognition without recognition, that is, without a constancy of some kind by which what is being known is recognized for what it is." And when we engage in identification, he adds, we recognize "a sameness beneath differences in space, time, or appearance—a One in the Many."[70]

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[62] Although in the pages to follow the components of ideology, as cultural phenomena, are analyzed separately, one should keep in mind that at the level of individual psychology these components appear to be subtly interconnected and interdependent. This is shown by Richard M. Merelman in his article, "The development of Political Ideology: A Framework for the Analysis of Political Socialization," *American Political Science Review*, 63 (September, 1969), 750–767.

[63] "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. Apter, pp. 206–261.

[64] Converse, p. 207.

[65] Converse, p. 229.

[66] Converse, p. 246.

[67] Converse, p. 214.

[68] Converse, p. 227.

[69] Converse, p. 232.

[70] *The Conduct of Inquiry: Methodology for Behavioral Science* (San Francisco: Chandler, 1964), p. 85.

At another, more self-conscious, level of understanding, explanation in natural and social science generally involves the practice of intentionally abstracting from the welter of events certain uniformities which are considered worthy of special attention. In political science, for example, Anthony Downs applies rationalistic constructs to the study of politics in a manner resembling the economists' hypothetical treatment of the "rational" consumer.[71] His method is first to abstract from empirical political behavior such key variables as political man's propensity to gratify his wishes through the utilization of appropriate means by gaining control over public policy. Downs then constructs a deductive model based on the assumption that political participants act according to this kind of "rationality." The model allows him to concentrate on certain prominent features of political action by abstracting these features and ignoring others. Hence, he argues:

> [W]e do not take into consideration the whole personality of each individual when we discuss what behavior is rational for him. We do not allow for the rich diversity of ends served by each of his acts, the complexity of his motives, the way in which every part of his life is intimately related to his emotional needs.[72]

This highly formalistic method may or may not be successful, but it is offered as an example of what any systematic theory attempts to achieve —an explanation that abstracts from complex phenomena by emphasizing their more important aspects, not their details.

The process of simplifying and ordering for purposes of explanation and understanding, however, extends beyond the practice of science to other areas of human thought. In the world of the citizen and politician, as in the world of the scientist, rationalistic constructs are necessary in order to comprehend the myriad events and activities which one seeks to understand. This process of abstraction is crucial in the political arena, where actors are continually forced to make policy choices based on their information about the situation. Either they must choose consciously to preserve, alter, or eradicate their social condition, or, without a political tradition, they drift willy nilly with circumstances over which they exercise no control. Henry David Aiken has argued, therefore, that

> simplification is an essential feature of any rational initiation of action. To refuse to simplify when one confronts a problem is in effect to reject the obligation to reach a solution; it is to make a game of possibilities and hence to move automatically outside the context of agency and choice.[73]

There are, of course, significant differences between science and ideology. In science one aspires to a self-critical, analytical, "objective" understanding of phenomena without reference to normative categories which assign value to alternative courses of action. Seeking to maximize intellectual clarity, science is not concerned with dilemmas of policy, choice or commitment. In politics, however, it is necessary to understand situations in terms of their moral significance for human beings, and the normative language of ideology provides reasons for supporting one social arrangement rather than another, reasons why this or that political program should be instituted, defended or abandoned. Science may be the final arbiter of an ideology's "factuality." Ideology, however, comprehends situations in a way which science cannot for ideology, unlike science, is able to portray the "facts" in terms of their relevance for human wants and aspirations.

This observation identifies another distinguishing feature of ideology: its role in the evaluation of policies and programs. Political ideology has particular relevance to the area of political agency because, in addition to providing a relatively structured and consistent conception of the causal forces operating in the social world, it also incorporates evaluations of what is conceived. Moreover, ideology conceptualizes and evaluates the contours of reality, not merely as it exists, but also as it might be shaped depending on the intervention of politically organized human beings in the historical process. And the power of these "experiments in the imagination" derives not only from their capacity to anticipate symbolically the physical features of a situation; their deeper significance lies in their ability to convey the human meaning of possible events or conditions. Aiken writes:

> I can assent to the proposition that on the first day of an atomic war every major city in the United States would be destroyed, without in the least *realizing,* in human terms, what the statement really means. In order that I may even remotely grasp such an idea, in absence of the event, I must somehow try symbolically to live through the horror and the agony of such a calamity. But this is precisely what the cold, literal, objective statement of fact does not require me to do.[74]

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[71] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957).

[72] Downs, p. 7.

[73] "The Revolt Against Ideology," in *Ideology, Politics and Political Theory,* ed. Cox, p. 154.

[74] Aiken, p. 153.

The language of science is too meager to capture the moral dimensions of such situations.

The evaluative component of ideology should not be equated with an "emotivist" view wherein ideologies mobilize mass publics through their power as "secular religion[s]" that both trigger and express "deep moral feelings,"[75] or by providing "ideological passion" that has the effect of "arousing and unleashing energies."[76] In philosophy, the "emotivist" view of normative propositions elaborated by A. J. Ayer and others,[77] argues that the only adequate test of a proposition's meaning is empirical verification; that normative terms, having no objects, lack any cognitive status; and that, lacking a cognitive role, such terms operate only to express and stimulate moral sentiment. Viewed as effects, the argument continues, normative terms are mere ejaculations of approbation and disapproval; conceived of as "causes," they are "triggering mechanisms" which elicit negative or positive responses from the recipient of value-laden communications.[78]

The contention that normative symbols operate solely to express feelings and arouse emotions, it may be admitted, has a certain *prima facie* appeal, because we are all familiar with instances in which certain symbols are met with a passionate expression of like or dislike, or cases in which strong feelings of attraction or revulsion for an object, event, or institution result in the expression of symbols that appeal to similar sentiments in others. I will not attempt here to consider whether such cases are adequately explained in terms of "causal" analysis, although I am inclined to believe that they are not.[79] I do contend, however, that these cases, and their explanation according to emotivist theory, do not take into account the extremely important role normative terms play in evaluation, especially the evaluations related to political action. For evaluation—the actor's appraisal of his environment and its potential for change in terms of his needs and aspirations[80]—is not the equivalent of expressing or arousing emotions, even though both activities involve the employment of normative terms.

An elaboration of this point will serve also to clarify another important feature of ideology—its action-component. Evaluation is "causally" related to purposive action in a much different way than symbols are said to "cause" political mobilization in the "emotivist" conception. In purposive explanation we do not, in fact, refer to antecedent events which are said to "cause" any kind of doing; rather one refers to conditions or events which the actor is attempting to bring about. Hence, "to explain by purpose is to explain by the goal or result aimed at, 'for the sake of' which the event is said to occur."[81] If we speak of common purposes, as we usually do in the discussion of ideologies, we mean that each member of a group "accepts a description either of a fairly specific state of affairs or of a more generalized objective (such as 'social security for all'), to be effected if possible through deliberating and acting together."[82] The importance of ideology for politics, it seems to me, resides in its power to communicate cognitions, evaluations, ideals, and purposes among members of groups. It enables them to appraise their political condition and its prospects for the future and thereby facilitates the mobilization and direction of energies and resources for common political undertakings. The significance of ideology in mobilization is not that it "causes one to do" but that it "gives one cause for doing." It provides grounds or warrants for the political activity engaged in. Reference to purposes and evaluations gives meaning to political actions, making them comprehensible to oneself and others. To explain the mobilizing aspects of ideology in terms of triggering mechanisms or the release of "ideological passion," therefore, ignores a vast and important aspect of how political actors understand and orient their political activities, and how observers, attempting to understand these activities, might explain their significance.

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[75] Bell, pp. 400–401.

[76] Sartori, pp. 403–405.

[77] For expressions of the "emotivist" position in ethics see A. J. Ayer, *Language, Truth and Logic,* 2nd ed. (New York: Dover, 1946), and Charles L. Stevenson, *Ethics and Language* (New Haven: Yale University Press, 1944). For the systematic use of this approach in the analysis of ideology, see Gustav Bergmann, "Ideology," *Ethics,* 61 (April, 1951), 205–218.

[78] See Bergmann, pp. 206, 211.

[79] See, for example, A. I. Melden, "Reasons for Action and Matters of Fact," *Proceedings and Addresses of the American Philosophical Association,* 35 (October, 1962), 45–60, and A. R. Louch, *Explanation and Human Action* (Berkeley and Los Angeles: University of California Press, 1966), especially pp. 134–158.

[80] My notion of evaluation is consistent with the more explicit but cumbersome definition of values proposed by Clyde Kluckhohn: *"A value is a conception, explicit or implicit, distinctive of an individual or characteristic of a group, of the desirable which influences the selection from available modes, means, and ends of action."* See "Values and Value-Orientations in the Theory of Action," in *Toward a General Theory of Action,* ed. Talcott Parsons and Edward A. Shils (New York: Harper Torchbooks, 1962), p. 395.

[81] Charles Taylor, *The Explanation of Behaviour* (London: Routledge and Kegan Paul, 1964), pp. 5–6.

[82] Dorothy Emmet, *Function, Purpose and Powers: Some Concepts in the Study of Individuals and Societies* (London: Macmillan, 1958), p. 113.

In the preceding pages some of the main factors in my conception of ideology—historical consciousness, cognitive power, evaluative power, and action-orientation—have been explored. The remaining prominent feature of ideology, its logical coherence, has been treated implicitly during much of the discussion, but several more explicit remarks regarding this criterion of ideology will be useful. By stipulating that ideology takes a logically coherent form, I mean to distinguish "constrained" ideologies from any random or inchoate ideas, attitudes, or feelings about politics. This is not to claim, as one might assume, that ideology observes the strictures of formal logic; ideology is not normally an exercise in syllogistic reasoning, and its arguments rarely proceed with "logical necessity," even if, occasionally, it may give that appearance. The logic of ideology is usually as broad as the conceptions, reasons, and justifications that typically "count" in the sociocultural structure (or, to use Wittgenstein's phrase, "form of life") within which the ideology operates.[83] Nevertheless, given these variegated logical resources (which include, but are always much broader than, those allowed by formal logic), ideology must not repeatedly violate their canons of sensibility. Within their confines the ideology must "make sense" and not result in logical absurdities. At one extreme, this conception of ideology will allow us to distinguish between it and the *ad hoc*, piecemeal appeals of propaganda. At another extreme, ideology becomes distinguishable from ambiguous and unarticulated conceptions such as ethos, spirit of the times, *Weltanschauung*, or public opinion.

In the foregoing analysis it is argued that ideology may be distinguished from other cultural forms by the combined presence of several elements—historical consciousness, action-orientation, cognitive power, evaluative capacity and logical coherence. Hence I would define ideology as a logically coherent system of symbols which, within a more or less sophisticated conception of history, links the cognitive and evaluative perception of one's social condition —especially its prospects for the future—to a program of collective action for the maintenance, alteration or transformation of society.[84]

It is hoped that this definition and the analysis on which it rests will help to clarify the concept of ideology, making it more relevant to the already substantial—and continuing—research concerning the nature and role of ideology in politics. Of course, before this concept of ideology can be made operational for empirical research, further conceptual refinement will be necessary. This paper proceeds on the conviction, however, that if the basic theoretical issues underlying a concept's usage are treated systematically and self-consciously, the empirical techniques and results which are shaped by that concept will be of greater merit. Certainly, in the case of ideology, an ordered attempt to give the concept empirical discriminating power by clarifying its relations to other phenomena of political culture is long overdue, and I hope that this paper has made some progress in that direction.

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[83] See Dallas M. High, *Language, Persons and Belief: Studies in Wittgenstein's Philosophical Investigations and Religious Uses of Language* (New York: Oxford University Press, 1967), pp. 211–212.

[84] This definition is, to varying degrees, in accord with the usage of some other contemporary writers on ideology in several academic disciplines. See: Aiken; Zbigniew B. Brzezinski, *Ideology and Power in Soviet Politics* (New York: Praeger, 1962); Converse; Friedrich; Geertz; Merelman; Joseph LaPalombara, "Decline of Ideology: A Dissent and an Interpretation," *American Political Science Review*, 60 (March, 1966), 5–16; Walzer, "Puritanism as a Revolutionary Ideology."

The observation of another writer on ideology would seem to indicate that the factors included in my definition have a wider acceptance: "Agreement on the meaning of the term [ideology] is far from universal, but a tendency can be discerned among contemporary writers to regard ideologies as *systems* of belief that are elaborate, integrated, and coherent, that justify the exercise of power, explain and judge historical events, identify political right and wrong, set forth the interconnections (causal and moral) between politics and other spheres of activity, and furnish guides for action." Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June, 1964), 362 (author's emphasis).

Now
Look
at p4

Preconditions of Mayoral Leadership*

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Introduction: The Search for Mayoral Leadership

After reviewing the problems which constitute the contemporary "urban crisis," most observers come to the conclusion that urban political leadership—usually mayoral leadership—is a crucial ingredient in a city's ability to deal with those problems. The President's Riot Commission was outspoken on this subject:

> Now, as never before, the American city has need for the personal qualities of strong democratic leadership. Given the difficulties and delays involved in administrative reorganization or institutional change, the best hope for the city in the short-run lies in this powerful instrument. In most cities the mayors will have the prime responsibility.[1]

For the Commission, the role of the mayor is an awesome one:

> As leader and mediator, he must involve all those groups—employers, news media, unions, financial institutions and others—which only together can bridge the chasm now separating the racial ghetto from the community. His goal, in effect, must be to develop a new working concept of democracy within the city.[2]

Proponents of mayoral leadership readily admit that few mayors have formal authority and resources which are adequate to deal with the enormous tasks facing them. But this is not considered an insuperable obstacle. Alexander George has written that

> The pluralistic structure of dispersed, decentralized power requires a political leader who can accumulate personal influence to supplement his limited formal authority. Studies of successful leaders in urban systems have led to the formulation of a simple, but incisive model of the "political entrepreneur"—a political activist adept at accumulating a variety of political resources and using them to gain influence and additional resources.[3]

An alert mayor can view political fragmentation as providing numerous opportunities for the "brokerage" function—mediating disputes, serving as a line of communication, and suggesting integrative solutions in which diverse groups can all gain. And in performing this function, the mayor finds his own power increased. "To the political entrepreneur, who possesses skill and drive, the pluralistic dispersion and fragmentation of power in democratic systems offer unusual opportunities for pyramiding limited initial resources into a substantial political holding."[4] Thus, effective political leadership in a fragmented system is proclaimed to be a necessity in American cities. And political scientists have provided a model of the broker-entrepreneur who can fill that need.[5]

It is easy to advocate "mayoral leadership" and "political entrepreneurship." But what does successful "leadership" mean in a modern American city? Andrew McFarland has defined leadership in the following way:

> A *leader* may be defined as one who has unusual *influence.* Influence may be viewed as one's capacity to make people behave differently than they would have otherwise. A leader may also be defined as one who has unusual *power.* Here we view "power" as a person's capacity to make others do something that they would not do otherwise and that the person specifically wants or intends.[6]

According to this social causation definition, a mayor may be said to exercise leadership when he alters human behavior in ways that he himself desires. Thus, he must set goals and influence others to aid in the pursuit of those goals.

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* This paper is based upon four years of participation in, and observation of, the mayor's office in Oakland. The research was conducted under the auspices of the Oakland Project at the University of California at Berkeley, headed by Aaron Wildavsky and funded by the Urban Institute. I would like to thank Professor Wildavsky. Robert Biller, William K. Muir, Jr., Bill Cavala, and Robert Nakamura for their comments.

[1] *Report of the National Advisory Commission on Civil Disorders* (New York: Bantam, 1968), p. 298.

[2] *Report of the National Advisory Commission on Civil Disorders,* p. 299.

[3] Alexander L. George, "Political Leadership and Social Change in American Cities," *Daedalus* (Fall, 1968), p. 1197.

[4] George, p. 1197.

[5] See Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), pp. 225–227, 308; Edward C. Banfield, *Political Influence* (New York: Free Press, 1961), chap. 8 and pp. 17, 309, 312–313, 320–321; Aaron Wildavsky, *Leadership in a Small Town* (Totowa, N.J.: Bedminster Press, 1964), pp. 244–245, 248; H. H. Gerth and C. Wright Mills, *From Max Weber* (New York: Oxford University Press, 1946), p. 109; and Andrew S. McFarland, *Power and Leadership in Pluralist Systems* (Stanford, Calif.: Stanford University Press, 1969), pp. 153–219.

[6] McFarland, p. 154.

Model of Mayoral Leadership. For those who call for strong and progressive mayors, a model of effective mayoral leadership should include a specification of: (1) the goals toward which a mayor might strive; (2) those groups in his environment which he must influence in support of those goals; (3) the ways in which he may influence them; and (4) the resources required for an effective exercise of power.

The goals toward which a model mayor might devote his energies and lead his constituency are numerous, varied, and difficult to accomplish. Writers on urban affairs have listed numerous possible goals: effective law enforcement; redevelopment; relocation of those living in redeveloped areas; expansion of the tax base; construction of low-cost public housing; improvement of the schools; creation of jobs and job training programs. Besides these substantive goals, the ideal mayor might strive to maintain within the political system a process of constructive dialogue between diverse groups which would contribute to harmony in the city.

In pursuing his goals, a mayor would have to exercise control and direction over the city council and relevant city departments: schools, redevelopment, housing, police, etc. The successful mayor would attempt to stretch his "legal" jurisdiction as far as possible. Furthermore, he would have to activate nongovernmental groups in the community to support his efforts by promising rewards and threatening sanctions. He would have to persuade businessmen to locate factories in his city and to initiate job training programs, and he would have to persuade labor unions to open places in apprenticeship programs to minority groups.

An ideal mayor would use his control of the mechanism of a political party to further his policy preferences, by supporting the nomination and election of candidates who were willing to support him. Furthermore, the model mayor would be willing to use publicity in order to appeal to the public to suppprt him against opponents by voting in elections or by trying to influence recalcitrant groups.

In order to exercise leadership over these various groups—in order to make them do what they would otherwise *not* do—the ideal mayor would require various resources. They would include:

(a) Sufficient financial and staff resources on the part of the city government;
(b) City jurisdiction in social program areas —such as education, housing, redevelopment, job training, etc;
(c) Mayoral jurisdiction within the city government in these policy fields;
(d) A salary for the mayor which would enable him to spend full time on the job;
(e) Sufficient staff support for the mayor—for policy planning, speech-writing, intergovernmental relations, and political work;
(f) Ready vehicles for publicity, such as friendly newspapers or television stations;
(g) Politically-oriented groups, including a political party, which the mayor could mobilize to help him achieve particular goals.

As this list suggests, the opportunities for mayoral leadership in a particular case may be limited by the political structure of a city (electoral system, parties, groups), by governmental structure, and by the lack of staff, jurisdictional and financial resources. Furthermore the quality of mayoral leadership may be strongly influenced by the personality of the mayor himself. Although social forces and governmental structures set limits to what *any* man might be able to do as mayor in a particular city, personal variability should not be discounted.[7] Thus in examining the quality of mayoral leadership, we must ask to what extent a given mayor's performance is influenced by social forces and governmental structure, and to what extent that performance is influenced by the mayor's personality.

Perhaps because of most mayors' lack of extensive governmental authority, observers of city politics have not concerned themselves with the problem of unrestrained mayoral leadership. But an enumeration of forces for restraint might include the mayor's dependence on independent groups for the attainment of his goals and the existence of competitive elections in the city.

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[7] Fred I. Greenstein has stated that the likelihood of personal impact on politics (1) increases to the degree that the environment admits of restructuring; (2) varies with the actor's location in the environment; and (3) varies with the personal strengths and weaknesses of the actor. See Greenstein, "The Impact of Personality on Politics: An Attempt to Clear Away Underbrush," *American Political Science Review*, 61 (September, 1967), 634. For Greenstein, personal variability is most likely to manifest itself in those fluid situations in which "political actors lack mental sets which might lead them to structure their perceptions and resolve ambiguities" (p. 638). In positions which are typically free from expectations of fixed content, personal variations will be more evident (p. 639). McFarland also notes the importance of an ambiguous environment; he sees leadership opportunities in "conflicting expectations among others about the person's behavior in a position." McFarland, p. 203.

This paper will explore the politics of mayoral leadership—or nonleadership—in a particular city: Oakland, California, under the administration of Mayor John H. Reading. The second part examines the often hostile environment in which the mayor works—the city of Oakland and its political structure, the city government, and the mayor's own office. The third part focuses on the personality of the mayor himself, using material drawn from speeches, interviews, newspaper articles, and personal observation. The last part analyzes the mayor's performance in light of the resources available (or potentially available) to him.

By examining mayoral leadership in a particular city, and by comparing that leadership to the model described above, this essay will suggest some of the difficulties surrounding urban leadership in an era of racial tension, confrontation, and insufficiency of urban resources. If we understand these difficulties, we will be better prepared to chart the preconditions for successful mayoral leadership.

Although some of the vital preconditions—financial resources, governmental authority, groups, etc.—may be missing, a mayor can still attempt to be a leader. But the style and consequences of his leadership depend to a large extent on the type of governmental and political system in which he operates; the concluding section of this paper will discuss some of the connections between political environment and leadership.

Environment

The City. Oakland's population in 1970 was 361,561, of which 34.5 per cent was black.[8] Studies have repeatedly shown that minority groups (blacks and Mexican-Americans) are heavily over-represented in both the poor and unemployed population of the city.[9] And Oakland's overall unemployment rate has reached a level more than twice the national average.[10] This is the kind of city which is discussed at length in the news media and in the growing "urban crisis" literature. It is characterized by an increasingly large minority population, by geographical separation and emotional tension between races, and by continuing problems of poverty and unemployment.

Political Environment: The "Non-Politics" of Oakland. One of the requirements postulated for successful mayoral leadership is the existence of "politically-oriented groups, including a political party, which the mayor can mobilize to help him achieve particular goals." Such groups and parties would give the mayor an opportunity to identify interests which had become aggregated by and articulated through organizations. Furthermore, groups and parties could perform the function of recruiting political leaders with whom the mayor could bargain. Finally, the mayor could use friendly groups as vehicles for political education in the goals toward which he was working and as organizing devices to bring citizens together in support of those goals.

In Oakland, a fundamental obstacle to mayoral leadership is the nature of the political environment itself. For this environment is characterized by a lack of interest in the electoral process and an absence of political parties and groups.

It often appears as though politics in Oakland does not exist. In 1969, when elections were held for mayor, city council, and the board of education, the candidate response was tepid at best. Two little-known men emerged to challenge Mayor Reading's attempt for reelection; two new candidates filed against two of the three incumbents on the school board; and all three city councilmen who were up for reelection were unopposed.

In a city marked by serious divisions, what causes the avoidance of electoral politics? Many answers have been suggested: the lack of resources and power in city government; the middle- and upper-class white bias of the at-large election system, which makes candidacies expensive and victories for nonwhites difficult to achieve; and, finally, the paltry salaries of elected officials.

Another reason for the anemia of Oakland's electoral process has been the noticeable absence of political party activity in municipal elections. This absence has, no doubt, been encouraged by the nonpartisan nature of the municipal ballot, which gives voters no indication of the party to which a candidate belongs. But formal nonpartisanship is not a sufficient explanation for party inaction; it has not prevented extensive activity by Democratic and Republican organizations in neighboring Berkeley and San Francisco.

A basic cause of party inactivity in Oakland has been the lack of organized politically interested groups in the city upon which a party or mayor could build. When an organization of high-school students tried to form a "coalition" in 1968 and 1969 to work for changes in school procedure and curriculum, they found, as one leader put it, "that there was no one to coalesce with." Labor has not played a significant role in city politics since the 1940s. And business groups, though formally endorsing certain nonpartisan causes like charter reform, have not heavily involved themselves in city politics.

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[8] Census report cited in the *Oakland Tribune*, March 14, 1971.

[9] See, for example, Survey Research Center, University of California, *Poverty and Poverty Programs in Oakland* (Berkeley, 1967) and Stanford Research Institute, *Human Resources Development for Oakland: Problems and Policies* (Menlo Park, Calif., 1968).

[10] Stanford Research Institute, p. 10.

The absence of active parties and groups, and of citywide politics itself, constitutes a grave limitation on mayoral leadership. Additional limitations are provided by the structure and process of the city government.

The Limits of City Hall Control. It is important at the outset to understand the jurisdictional limits of city government in Oakland. For only a few of the problem areas that we usually associate with the "urban crisis" are directly under the control of the city council and city manager. The school system, for example, is completely independent of city hall, except for the provision that the city council may pass a 5 per cent "emergency tax" for the schools. Another powerful independent body is the Oakland Board of Port Commissioners, which has exclusive control over management of the massive Port of Oakland.

Although redevelopment and housing have been two of the most controversial policy areas in Oakland, the commissions that have jurisdiction in these areas are the autonomous Redevelopment Agency and Housing Authority. These two organizations have had strained relations with each other, as well as with the city government.

Finally, the local poverty agency, the Oakland Economic Development Council, Inc., has been militantly independent of city hall since 1967. This agency operates a host of OEO programs, as well as the Department of Labor's Concentrated Employment Program.

The implications of fragmentation for mayoral leadership are fairly serious. Without jurisdiction, a mayor may be powerless to control outcomes in policy areas which are important to him. In Oakland, the mayor has been disturbed both by the slow pace of renewal and by the lack of relocation opportunities caused by the slowness of progress in public housing. But he has encountered difficulties in communicating with the two autonomous agencies involved, let alone in forging a bond of cooperation between them. Although a charter revision in 1968 brought the parks, recreation, library, and museum commissions more closely under city hall's control, the critical agencies discussed above remained autonomous.

Now that we have considered some of those governmental bodies which are outside city hall, let us look more closely at the inside of city government itself.

The Separation of Politics from Administration. In a seminal article which first appeared in 1887, Woodrow Wilson declared that "administration lies outside the proper sphere of *politics*. Administrative questions are not political questions. Although politics sets the tasks for administration, it should not be suffered to manipulate its offices."[11] Politicians should set policy, and scientific administrators should carry it out. This separation of politics from administration, a hallmark of the Progressive era in American politics, lies at the root of the council-manager form of government. In Oakland, according to the city charter: "The Council shall be vested with all powers of legislation." The charter goes on to state, however, that the council "shall have no administrative powers."[12] These are reserved for the city manager, who has "administrative" power over city government departments—police, fire, street and engineering, parks, recreation, and so on.

As for the mayor, who is a member of the council, the charter makes his duties sound impressive:

> The Mayor shall be the chief elective officer of the City, responsible for providing leadership and taking issues to the people and marshalling public interest in and support for municipal activity. He shall recommend to the Council such legislation as he deems necessary and shall encourage programs for the physical, economic, social and cultural development of the City. He shall preside over meetings of the Council, shall be the ceremonial head of the City, and shall represent the City in intergovernmental relations as directed by the Council.[13]

In theory, the mayor leads public opinion, the council formulates policy, and the manager carries it out. But in practice, the relationship between politics and administration in Oakland is strongly affected by the personalities of the men holding city offices and by the wide disparity in the levels of resources available to the politicians on the one hand and the administrators on the other.

An examination of comparative resources helps to explain why there is a strong tendency in Oakland for administration to devour politics—for the mayor and council to follow the manager's lead in the policy-making process. Councilmen, who earn $3600 annually from the city, and the mayor, who is paid $7500, find it financially difficult to spend much time at their jobs; the manager's annual salary of $38,940 permits him to do so. The entire city council is served by one secretary, and the mayor is not much better off with three secretaries and an administrative assistant. But the city manager, besides having three full-time staff assistants, is able to utilize the manpower and informational resources of all city departments under his control. As a result, the city manager tends to know more than anyone else about city government structure, processes, and substantive policy.

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[11] Woodrow Wilson, "The Study of Administration," reprinted in *Political Science Quarterly*, 56 (December, 1941), 494.

[12] *Oakland City Charter*, Sec. 207.

[13] *Oakland City Charter*, Sec. 219.

A potential monopoly on staff and informational resources is not, however, the only reason for city manager domination of the Oakland governmental process at the present time: Jerome Keithley, the current city manager, is a man who is unusually adept at dealing with the internal environment of city government. The city council's power to fire the city manager gives councilmen an ultimate trump card to play against the manager, but Keithley's performance in his job has gained him the solid and enthusiastic support of the council. There are a number of reasons for this support.

First of all, Keithley shares the views of most councilmen about what the goals of city administration should be: efficiency, cost cutting, and thus lower taxes. The city manager defines his role as an inside administrator, avoiding ambitious social programs. Secondly, City Manager Keithley uses his access to departmental records to satisfy as quickly as possible councilmen's requests for information. And he permits councilmen to make decisions in areas in which they are particularly interested. For example, the city manager allows the council to have jurisdiction over the budget for "civic organizations" such as the Chamber of Commerce, the Elks Club, the Italian-American Federation, and the California Spring Garden and Home Show Committee. Finally, Keithley builds support on the council by being "one of the guys." He is a regular attender at city council dinners and at ball games at the Coliseum—events that the mayor often avoids.

Armed with his considerable advantages, the city manager defines "policy" and "administration" in such a way that "administration" turns out to bulk very large and "policy" very small. When Mayor Reading complained in July 1968 that the police chief's stringent restrictions on policemen's use of guns (a decision in which the city manager had concurred but about which the mayor had never been consulted) constituted a dangerous policy decision which should be overturned by the council, the city manager disagreed. The manager's reasoning was clear: "A policy decision would be that policemen in Oakland should carry guns. Administrative decisions would be when they should carry guns, where they should carry guns, and how they should use those guns."

The city manager's definition of policy and administration is usually supported by the city attorney, Edward Goggin, who is appointed by the council but who takes his lead from the city manager.[14] Goggin has a penchant for defining the area of permissible action by the city government as extremely narrow; within the government, Goggin tends to rule against city council jurisdiction. When the mayor sought to investigate his supposed new powers under the Green Amendment to the poverty program, the city attorney advised him that the amendment —which allows city governments to take control of poverty programs—could not apply to Oakland. For the legislation provided that, in taking over the new jurisdiction, a city council would administer the program. And Goggin pointed out that the Oakland City Charter specifically prohibits the council from exercising administrative power.

The Typicality of Oakland's Governmental System and Electoral Rules. Table 1 shows the form of city government for cities with more than 5000 population. For cities in Oakland's population group (250,000–500,000), the council-manager system is the most popular form of government. This plan is particularly prevalent in states along the Pacific coast; 84.3 per cent of the cities (above 5000) in those states have adopted the council-manager plan.[15]

As to electoral rules, Oakland's nonpartisan form of local elections is widely used by cities in its population group; 76 per cent of cities in that range do not list parties on the ballot.[16] But in another respect, Oakland's electoral system is unique. Seven of the city's nine councilmen must reside in and run for office from particular geographical districts; one councilman

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[14] For a description of the crucial role in policy making that can be played by a city attorney, see "The Cambridge City Manager" in *Public Administration and Policy Development: A Case Book*, ed. Harold Stein (New York: Harcourt Brace and Company, 1952).

[15] *The Municipal Year Book, 1968*, p. 54.

[16] *Municipal Year Book, 1968*, p. 58.

Table 1. Form of Government in Cities Over 5,000 Population

Population Group	No. of Cities Reporting	Mayor-Council No.	Mayor-Council %	Council-Manager No.	Council-Manager %	Commission No.	Commission %
More than 500,000	27	22	81.5	5	18.5	—	—
250,000 to 500,000	27	11	40.7	13	48.2	3	11.1
100,000 to 250,000	93	33	35.5	50	53.8	10	10.7
50,000 to 100,000	215	83	38.6	116	54.0	16	7.4
25,000 to 50,000	439	166	38.2	233	53.6	40	9.2
10,000 to 25,000	1072	511	47.7	488	45.5	73	6.8
5,000 to 10,000	1112	686	61.7	378	34.0	48	4.3

Source: *The Municipal Year Book, 1968* (Washington, D.C.: International City Managers' Association), p. 54.

may reside anywhere in the city; and the mayor, who may live anywhere in the city, is elected separately. But each council seat is voted on by the city at large. Oakland is the only city in its (250,000–500,000) population group to "nominate by wards and at large, elect at large."[17]

The City Council. The segment of the political environment closest to the mayor is the city council, of which he is a member. And the council is a critical element affecting the mayor's ability to exercise leadership. For if the mayor could persuade four councilmen to go along with him, he could direct the city manager and attorney to follow his policy leadership—or he could fire the city manager and hire a new man who would work toward his goals.

City council recruitment has not been closely linked to elections. "Appointment politics" has become a tradition in Oakland; of the present members of the council, 6 were originally appointed, and only 3 were elected. Table 2 shows that this pattern of council recruitment has been a popular one in the city.

Once on the council, incumbents tend to be able to survive subsequent elections with ease. In the past 16 years, no "appointed incumbent" has lost the ensuing election. Overall, in the period between the spring elections of 1953 through the spring elections of 1969, only a handful of incumbents have lost. Table 3 shows the enviable electoral record of Oakland's incumbents.

Official city council meetings take place on Tuesday evenings and Thursday mornings, but most decisions are made at special "work sessions" before the council meetings—in the mayor's office on Thursdays and over dinner on Tuesdays. Although citizens' groups and the press have raised pointed questions about the secrecy of such meetings, both the mayor and council have felt that they can be more honest with each other at such sessions and also be shielded from pressure. The model of mayoral leadership specifies that the mayor must direct both the council and public groups, but the conflicts over secrecy have shown that it may be difficult to do both at the same time. For the mayor is well aware that closed meetings facilitate informal discussion and the forging of agreement among council members, but the nonpublic nature of those meetings makes it difficult to generate either interest in or support for city policies among Oakland citizens.

Responses to Conflict: The "Shrinking Violet" or "Retrenchment" Syndrome. On the whole, the social and political attitudes of the councilmen tend to be conservative; these men are distrustful of governmental action in social areas and disturbed by the growing militancy of black people. At times, the council seems to pretend that Oakland's problems do not exist. In July, 1968, the Coordinating Community Council (made up of 16 school-community councils) asked the city council to levy a 5 cent emergency tax to lease portable classrooms, so that 19 elementary schools could eliminate double sessions. The chairman of the council's Ways and Means Committee admitted that the city council had the right to levy such a tax, but he noted that "what constitutes an emergency is a matter of opinion." He pointed out that, historically, an emergency in the schools had meant a "fire or catastrophe." Overcrowding and double sessions were not in that category, and the council did not accede to the request.[18]

If there is one thing that Oakland city councilmen do not like, it is conflict. And they have become adept at conflict avoidance, a process which is discussed in the literature of organization theory. Anthony Downs describes the "shrinking violet syndrome," in which a bureau avoids conflict by narrowing its actions and affecting fewer external agents.[19] And Matthew Holden, Jr., in an article on " 'Imperialism' in Bureaucracy," states:

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[17] *Municipal Year Book, 1968*, p. 59.

[18] *The Montclarion* (Oakland neighborhood newspaper), July 24, 1968.

> Perhaps the most neglected cases are those in which there is a clear disposition toward retrenchment or self-limitation. It is not merely that the agency adopts kid-glove tactics in order to maintain a cooperative relationship with a constituency, but that it actually denies its powers.[20]

In order to avoid the conflict, uncertainty, and turbulence of a hostile urban environment, the Oakland City Council goes beyond merely narrowing its actions and denying its powers. Indeed, the council follows a consistent policy of actively giving up jurisdiction in areas which it considers troublesome. After turning down the mayor's proposal to appoint an ombudsman for the city in 1967, the council hit upon a painless way to solve the problem of citizens' grievances. For the low price of $10,000, the city was able to enter into an agreement with Alameda County whereby the county's (powerless) Human Relations Commission would add another member to its staff—with that member concentrating his efforts in Oakland. The city government was able to avoid responsibility for setting up grievance procedures; restrictions on the county commission's activity made it unlikely that any administrator would be embarrassed or threatened; and the cost to the city was only $10,000.

Avoidance of conflict should not be confused with passivity or openness to mayoral initiative. For some mayoral initiatives—for example, a move for greater control of the poverty program—would mean more conflict for council members. Furthermore, the council tends to resent any special privileges available to the mayor; council criticism of the mayor's staff has been common.

Table 2. Prevalence of Appointment Politics in Oakland, 1953-1968

Office	Number of Changes	Originally Appointed	Originally Elected
Mayor	2	1	1
Councilman	17	9	8

Source: Oakland Citizens for Responsive Government, "Effect of Appointments Upon the Oakland Governmental Structure" (pamphlet published in June, 1969).

Table 3. Success of Incumbents in Oakland Politics, 1953-1969

Office	Number of Incumbents' Campaigns	Number of Incumbents' Victories	Number of Incumbents' Defeats
Mayor	6	5	1
Councilman	34	29*	5

* 12 of these were unopposed.

Source: Oakland Citizens for Responsive Government.

Even the mayor's own office in City Hall is not necessarily friendly terrain. When Mayor Reading discovered in the fall of 1967 that his receptionist was openly critical of him and his policies, the mayor decided to transfer her to another department. But the city manager, fearing the wrath of the employees' association, advised caution, and civil service moved slowly in arranging the transfer. It was not until the spring of 1969—a year and a half later—that the change was made.

Such an environment would test the mettle of any mayor. But what manner of man is John H. Reading? The next sections will examine Reading's personality and his use of resources. By comparing Reading's actions with those of his predecessor, John C. Houlihan, we can suggest how much difference personality makes in the Oakland mayor's office.

The Mayor Himself

John Reading's life has been marked by a number of significant successes. As an air force pilot in World War II, he rose to the rank of Lt. Colonel. As a businessman, he has greatly expanded his family's food processing business and has become one of the leading manufacturers of tamales in the country.

In 1961, Reading was appointed to the city council. Mayor Houlihan had noticed Reading's attendance at, and participation in, meetings concerned with various neighborhood and civic issues. Feeling that Reading's comments were sensible and that his enthusiasm for "civic involvement" was strong, Houlihan asked Reading if he would like to join the city council. Reading was reluctant at first, but he felt that his advocacy of civic involvement meant that he should take the responsibility. (The appointment of a man who was not active in party politics or political groups, but just an "interested citizen," is indicative of the nonpolitical, nonactive-group nature of the Oakland system.) In the spring of 1966, when Mayor

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[19] Anthony Downs, *Inside Bureaucracy* (Boston: Little, Brown and Company, 1967), p. 217.

[20] Matthew Holden, Jr., " 'Imperialism' in Bureaucracy," *American Political Science Review*, 60 (December, 1966), 945.

Houlihan resigned in the wake of an embezzlement scandal arising out of his law practice, Reading was chosen by the council to be the new mayor. He was overwhelmingly reelected in 1967 and 1969.

A Private Man in a Public Office. Discussing the "purist" style in American politics, Nelson Polsby and Aaron Wildavsky state that such a style "represents a virtually complete privatization of politics" in which

> the private conscience of the leader—rather than his public responsibilities—becomes the focal point of politics. Internal criteria—possession of, devotion to, and standing up for private principles—become the standards of political judgment. Constituents disappear, and we are left with a political leader determining policy on the basis of compatibility with his private principles.[21]

In John Reading's view standards of conduct which are applicable to man's behavior in private life, embodying values of truthfulness, etiquette, and respect, should be applicable to the public sphere. If absolute openness is desirable in private life, then it is also desirable in politics. When a black community leader agreed with Mayor Reading on certain issues in private and then attacked the mayor in a public meeting before a black constituency, the mayor felt constrained to remind the community leader—publicly—of all he had said in private meetings. After all, reasoned the mayor, there should be no dividing line between private and public.

Mayor Reading finds himself facing a fundamental dilemma: He is a political leader who does not like politics. Talking about his experience at the University of California and afterward, the mayor once remarked: "I was never interested in political science, or any aspect of the political arena at all. But I am interested in business and modern management techniques —you know, span of control and that kind of thing. I wanted to put business management in practice in government." But the going has not been easy; Reading confessed that "as mayor, I've found that there isn't enough information or control to run government like a business. All we can do is just muddle through."[22]

The Consequences of a Businessman's Approach to Politics. Mayor Reading's attempt to bring private and business standards into politics has had a number of consequences for his behavior as mayor. First of all, this approach has led him to try to simplify politics, to make governmental action clearer and more rational. He has consistently argued for the formation of "superagencies" within the city government, which could "coordinate" the tasks of existing departments. Reading talks about coordination as a technical exercise; a more public mayor might recognize that, when different organizations have different preferences, "coordination" between them might mean that one wins and the other loses.

A second consequence of the mayor's private-business approach is his tendency to define public problems in economic terms:

> I am convinced that employment is a factor of economics much more than of civil rights. Granted there may be areas of discrimination directly or inadvertently and we should continue breaking barriers, urging realistic employment standards and emphasizing full opportunity. But in my experience many of the unemployed are just not adequately prepared to take advantage of those opportunities.[23]

In the mayor's view, education and employment, not political organizing, are the keys to ending poverty.

Another outgrowth of the mayor's private attitude to politics is his tendency to deal with political problems in a moralistic way. People who take nonmoral social actions should be punished for them. When City Manager Keithley discussed the possibility in 1969 of levying a bar drinker's tax to help pay the costs of additional policemen, the mayor responded: "That's only fair. This links the issues nicely! People who drink at bars should bear the costs for protection from crime."

On law and order the mayor is uncompromising. Responding harshly to demands for decentralization and community control of the Oakland Police Department, Mayor Reading declared:

> Some persons . . . talk of harassment and brutality by policemen who are sworn to defend and protect this community with their lives. They call for creation of locally elected police control commissions which would be empowered to patrol and control the police. What they really are asking is the elimination of the police, for the proponents of this plan obviously advocate anarchy.[24]

Although the mayor often defends the status quo, it should not be thought that he is insensitive to individuals' grievances against the government. In fact, his private-business orientation has led him to be suspicious of unrestrained governmental power. In an effort to protect individual rights, he has fought—unsuccessfully—for an ombudsman plan in Oakland.

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[21] Nelson W. Polsby and Aaron Wildavsky, *Presidential Elections* (New York: Charles Scribner's Sons, 1968), p. 180.

[22] Informal remarks at meeting of San Francisco Bay Area Chapter of the American Society for Public Administration, Oakland, October 5, 1967.

[23] Remarks before U.S. Civil Rights Commission, May 6, 1967.

[24] Remarks at Oakland City Council Meeting, April 23, 1968.

Responses to Conflict. Like the city council, Mayor Reading dislikes conflict. "I'm thankful that there's no dissent on our council. When you look at Berkeley, there are enormous divisions." The question that naturally arises is why a man who operates so successfully in the competitive world of business finds it difficult to deal with conflict in politics. A possible explanation is that Reading's success in his food business has been caused in great part by his discovery of a method of manufacturing tamales that was significantly cheaper than his competitors'. Thus, he won a technological competition, not a conflictful interpersonal one.

Recognizing that differences of opinion do exist in public life, Mayor Reading tries to deal with them by using what might be called the "big round table" method. He believes that if people could just get together on a personal level, around a table, and talk over their differences, then everything could be settled. But a "public" mayor, one who wishes to exercise leadership in accordance with the model outlined in the first section, might have much to gain from a different view: a recognition of the fact that conflicts exist and may be taken advantage of by leaders. McFarland[25] views leadership in a pluralist system as an individual's response to multilateral conflict. When conflict increases, set expectations break down and the environment may be manipulated by the leader. A mayor may serve as mediator between conflicting groups; he may seek to "upgrade the common interest" (as Mayor Lee did in New Haven by creating an urban renewal program that unified otherwise conflicting interests); or he may take advantage of a conflict situation to support the party with whom he agrees. All of these strategies are impossible if the mayor does not recognize, face, and deal with conflict situations.

The problem of current city politics, according to Mayor Reading, is that certain groups—most notably black militants—will not agree to set aside their own selfish interests and work for the community interest. And the losers are inevitably the "large, inactive majority . . . who would like to see real, lasting, and reasonable solutions to our problems." These people "go about their business, raise their family, take their tax lumps" and are currently "taking a back seat" to the militants in public dialogue.[26] The mayor has particular sympathy for Oakland's old people, who are living on fixed incomes and worried by crime.

Confrontation politics, with threats, demands, and insults—which Reading felt as personal attacks—took a tremendous toll on the mayor during disputes about the poverty program, police, and model cities in 1968. Depressed, he tentatively decided not to seek reelection in 1969. But the passage of a new charter, the election of fellow-Republican Richard Nixon to the Presidency, and, perhaps most important, "a look at the alternative candidates" convinced the mayor to run again. Civic responsibility had once again overcome personal predilection.[27]

Now we must ask what Reading can do as mayor, and what a different kind of man might do in this difficult governmental and political structure.

Resources

As we have seen, students of mayoral leadership have focused on the wise use and "pyramiding" of resources as ways in which an urban political leader can gain power. Dahl notes that

> Although the kinds and amounts of resources available to political man are always limited and at any given moment fixed, they are not . . . permanently fixed as to either kind or amount. Political man can use his resources to gain influence, and he can then use his influence to gain more resources. Political resources can be pyramided in much the same way that a man who starts out in business sometimes pyramids a small investment into a large corporate empire.[28]

Oakland seems to offer few opportunities for a mayor to pyramid political holdings. But a mayor of Oakland does have some resources, or at least potential resources. They are:

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[25] McFarland. *Power and Leadership* . . . , pp. 177 ff.

[26] Mayor's Inaugural Address, July 1, 1969.

[27] In an essay entitled "Political Incentives," James L. Payne and Oliver Woshinsky discuss, among other types, politicians who are motivated by an "obligation" incentive. Such people enter politics because they feel a duty to do so, and they are preoccupied with upright conduct in accord with fixed principles. Like Mayor Reading, they do not enjoy politics and have a great distaste for compromise. (Unpublished essay, April 1970). James David Barber describes a similar type—he calls them "Reluctants"—in his study of Connecticut legislators. The author found that many Reluctants entered politics because of a sense of duty, though they had a distaste for conflict and for politics. See Barber, *The Lawmakers* (New Haven: Yale University Press, 1965), pp. 130–154.

[28] Dahl, *Who Governs?* p. 227.

(1) *The mayor's position as president of the city council:* As mayor, Reading presides over public council meetings and private "work sessions." He also appoints council committees. Thus, he has an opportunity to exert a strong influence on the subjects that come up before the council and the ways in which they are discussed and decided.

Former Mayor Houlihan—an outspoken, gregarious lawyer who loves politics—enjoyed this power and exploited it. A careful counter of council votes, he set about to secure outcomes favorable to himself. Houlihan often dispensed favors to friendly councilmen; for example, he gave certain members of the council the opportunity to borrow the mayor's limousine. Once, in order to please a councilman whose chief interest lay in an airport golf course, the mayor created a Golf Course Committee and made the councilman its chairman and only member.

Unlike City Manager Keithley, who uses informal council gatherings to strengthen his position with that body, Mayor Reading avoids council dinners and trips with councilmen to ball games. He appoints council committees on the basis of interest and expertise, and he makes little attempt to influence their deliberations. The mayor is generous in his regulation of the council agenda. At a 1969 work session, Reading told the council that "committees should speak up when they have a report. I've heard complaints that I haven't notified committees that they may give reports, but it's your initiative, and any time you have a report, just speak up." In sharp contrast, Mayor Houlihan's practice was to give councilmen specific assignments and call upon them for reports.

Further complicating the mayor's task in leading the council is his unwillingness to count votes and to persuade councilmen to see things his way. That would be "pressuring," an activity the mayor does not regard as legitimate. Though Mayor Reading enjoys considerable respect on the part of the council, his failure to influence committee work, control the agenda, and employ persuasion have led him to an underutilization of the political resource of his city council leadership.

(2) *Power of appointment:* The mayor appoints (with the approval of a majority of the council) members of city boards and commissions—including the Housing Authority, Redevelopment Agency, and Port. But Reading has often appointed people he likes and admires personally—only to find out after their appointment that they do not share his policy preferences. And the mayor's unwillingness to "pressure" the city council has also restricted his influence in the appointment process. In July of 1968, the council refused to approve the mayor's reappointment of two members of the Planning Commission—one of them the chairman. At the same time, the mayor's move to replace an anti-Reading councilman's business associate as head of the Housing Authority failed when the man simply refused to leave his post and was backed up by some councilmen. In neither of these cases was Mayor Reading willing to fight on the council or in the newspapers for his appointments.

(3) *Prestige:* Describing the success of Mayor Lee of New Haven, Dahl shows how the mayor was able to use his personal popularity and prestige to extract concessions from political and business leaders.[29] Richard Neustadt says that "popular prestige" in the form of "public standing" is a source of influence for a President in his dealings with people in Washington. This is because political actors depend on the public for votes and support.[30]

Mayor Reading has had an enviable electoral record—amassing more than 80 per cent of the vote in 1967 and more than 63 per cent in 1969. The prestige that results from electoral success in the city's highest elective office might be turned into an important resource; Mayor Lee was certainly able to capitalize on his success at the polls. But in Oakland, the lack of interest in electoral politics renders this resource much less useful. Mayor Reading has been able to use the prestige of his office in certain instances: in organizing business leaders into a campaign for a school tax, in promoting support for a downtown hotel and convention center, and in leading the campaign for a new charter. But on the whole, the mayor has tended to underutilize this potential resource. For the existence of prestige without the willingness to use the next named resource, publicity, may not be sufficient to accomplish mayoral goals.

(4) *Publicity:* In a discussion of protest groups, Michael Lipsky has demonstrated how important publicity is to "low resource" political actors:

> The communications media are extremely powerful in city politics. In granting or withholding publicity, in determining what information most people will have on most issues, and what alternatives they will consider in response to issues, the media truly, as Norton Long has put it, "set . . . the civic agenda." Like the tree falling unheard in the forest,

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[29] *Who Governs?* p. 309.

[30] Richard Neustadt, *Presidential Power* (New York: John Wiley and Sons. Inc., 1960), pp. 86 ff.

there is no protest unless protest is perceived and projected.[31]

Like Lipsky's protest groups, Mayor Reading lacks adequate money, staff, and organizational foundation. But he has certain important advantages: widespread respect, the forum of city hall, and a friendly newspaper in the *Oakland Tribune.*

These advantages are not useful unless capitalized upon, and the mayor's essentially private nature and preference for quiet, rational discussion make him less than eager to take his case on public matters to the people through the news media. In May 1969, when the school board disregarded its own biracial citizens' screening committee in selecting a new superintendent (who turned out to be involved in a conflict-of-interest case elsewhere), the community was in an uproar. Sensing public outrage, the mayor sought to persuade the board to cancel an upcoming school tax election and to initiate immediate negotiations with community groups to try to rectify the situation. Although he was just one vote short, in a secret meeting, of influencing the school board to take his advice, the mayor adamantly refused to put the board on the spot by making any sort of public statement. The tax election ended in overwhelming defeat, and the mayor lost a golden opportunity to appear as a successful mediator.

Publicity is a resource which is not severely limited by considerations of formal structure and jurisdiction, and an enterprising mayor can make good use of the media. Mayor Houlihan had a 10-minute weekly radio program, through which he told listeners about his activities and current governmental issues in Oakland. When the city council refused to endorse a bond issue for Bay Area Rapid Transit, Houlihan came out publicly for the issue anyway. The bond campaign was successful in a close election; a switch of 2,000 votes in Oakland would have defeated the issue. In this resource area, a "public" personality can make a difference.

Underutilization and Dispersion of Resources. Operating in a low resource environment, Mayor Reading has tended to underutilize the political resources he has. For example, the mayor's failure to use persuasion has caused him to gain less than full advantage from his role as president of the council. Instead of pyramiding resources, he often appears to be engaged in *resource dispersion,* failing to combine his resources with each other. Thus, his power of appointment has been blocked by his failure to lead the council and to use publicity; and the mayor's personal prestige has been underutilized because of his unwillingness to take issues to the public. Except for occasional pronouncements on law and order, the mayor's private style has led him to avoid the public spotlight. For a low resource political actor who enjoys considerable personal respect, such avoidance seems unwise.

Personal Opportunities in Spite of Structure. Although governmental and political structures set limits on what any mayor can do, personality can make a difference. On the basis of Mayor Houlihan's experience and studies of successful leadership elsewhere, we can suggest some of the ways in which a more politically oriented mayor might use his leadership resources in Oakland. In *The Washington Community, 1800–1828,*[32] James S. Young describes a situation in the nation's capital at that time which appears remarkably similar to that in Oakland today: a lack of active groups; fragmented social subgroups formed around separate branches of government, with few integrating forces; weak congressional parties; an anti-political ideology, even among politicians; and an absence of presidental staff. Here, according to Young, the personality of the President made a difference. For "the President had to improvise out of wit and ingenuity, as his political talents, circumstances, statutes and good fortune permitted."[33] During the period studied, only Jefferson was successful enough at "statecraft" to be able to provide leadership. He utilized a combination of "social lobbying" (mainly intimate White House dinners), personal persuasion, and the newspaper forum provided by a publisher who was a friend of the President's.

A mayor of Oakland can also use "social lobbying" and personal persuasion as techniques to influence councilmen to follow his leadership. Mayor Houlihan, as we have seen, delighted in providing special favors for friendly councilmen, and City Manager Keithley uses dinners and sports events to good advantage. Social lobbying is not an activity Mayor Reading enjoys, and his distaste for "pressuring" councilmen leads him to avoid attempts at persuasion. But if the mayor did control a majority on the council, he could then direct the work of the city manager and attorney. When Mayor Houlihan was involved in drawing up a contract for Oakland participation in building the Coliseum Complex, he had the city attorney deliver legal opinions which supported the mayor's preferences.

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[31] Michael Lipsky, "Protest as a Political Resource," *American Political Science Review,* 62 (December, 1968), 1151.

[32] New York: Columbia University Press, 1966.

[33] Young, p. 159.

Just as Jefferson used a friendly newspaper to present his program, so Houlihan used a radio program to publicize his point of view. When he could not get the council to go along with him—as in supporting rapid transit bonds—he took his case to the people. And when city resources are inadequate, a mayor can try to persuade outside bodies to help. Thus, Mayor Houlihan was able to convince the Alameda County Supervisors to grant $350,000 from county highway funds to build a tunnel under the new Oakland museum.

Faced with a lack of groups in the political environment, a mayor can act to stimulate the creation of groups. When Mills College trustees informed Mayor Houlihan that construction of the potentially noisy MacArthur Freeway would force the college out of Oakland, the mayor decided to forestall the college's move by seeking a ban on truck traffic on that freeway. He formed a citizens' committee to support such a ban and joined them in pressing the case in the state capital. The state government decided to ban trucks; Mills stayed in Oakland.

Although mayors in Oakland are limited both by governmental structure and political disorganization, there are still opportunities for an inventive political man to make the most of the situation—by social lobbying, persuasion, publicity, and the creation of groups which will support him. But, as James Young has noted, all this requires adroitness, ingenuity, and political expertise. Even given Oakland's constraining structure, personality can make a difference.

American cities are unlikely to find their revenue constraints greatly eased in the near future, and the jurisdictional fragmentation about which so many mayors complain is not likely to disappear. Thus, the techniques of low resource leadership described here appear to have wide applicability.

Conclusion: Mayoral Leadership and the Political System

Although calls for "mayoral leadership" fill the literature on city politics, an understanding of such leadership in any given city requires an examination of the political environment, the personalities involved, and the ways in which the mayor uses the resources available to him.

It is clear that the leadership exercised by Mayor John Reading of Oakland does not closely approximate the model of leadership described in Section I—the model of a mayor who sets goals and then uses a wide variety of means to influence others to act according to the mayor's own preferences. This is due both to environmental and personal factors.

The introductory section listed seven environmental requirements for effective mayoral leadership, and it is obvious that the Oakland governmental and political structure provides little comfort for any mayor. Like most cities, Oakland faces a revenue strain and rising personnel costs—which means that it does not have (a) *sufficient financial resources* with which a mayor can launch innovative social programs. Nor does the city have (b) *jurisdiction* in the vital program areas of education, housing, redevelopment, and job training. Under the council-manager form of government, there are severe limits to (c) *mayoral jurisdiction within the city government*. And the mayor had neither (d) sufficient *salary* ($7500 per year) nor (e) adequate *staff* (one administrative assistant and three secretaries) to perform as a full time and well-informed official.

Although Oakland provides the mayor with (f) *a ready vehicle of publicity* in the *Oakland Tribune*, the rest of the political environment offers him little comfort. There are few (g) *politically-oriented groups* which can provide support for a mayoral program.

Without governmental jurisdiction, staff, and financial resources, it is hard for any mayor to direct, or even influence, the actions of others. But it is not true that mayoral leadership is impossible in Oakland. For the mayor does have certain resources (position as presiding officer; power of appointment; prestige; legitimacy), and an adept political man can pyramid those resources. John Houlihan, a man who enjoyed the turbulence of the public arena, was able to build on his meager mayoral resources. But John Reading, a private man with a distaste for conflict and a preference for private discussion, has consistently underutilized his political resources. In Oakland, a mayor's lot is not a happy one, but it is not a hopeless one, either.

As in other cities, the mayor of Oakland is in large measure the product of the political system. The nature of that system has a significant influence on the kinds of people who become mayor and the forms of leadership they can exercise. In the nongroup, nonpartisan, and nonelectoral political system of Oakland, it is entirely possible for a "nonpolitical" civic reformer like John Reading to attain the position of mayor without really trying for it. No groups, parties, or politicians could make an effective claim on the office. Even John Houlihan, who stands out among recent Oakland mayors as an ambitious politician, was promoted for the office by a loose coalition of citizens' groups representing many political views. Although Houlihan sought to be a full time mayor, the low salary forced him into deep financial trouble and hence led to his eventual downfall. Thus, the Oakland governmental system may be said to have penalized him for trying to exert sustained leadership.

In a governmental system in which a mayor lacks administrative authority, the leadership most often possible is hortatory in nature (encouraging voters to pass a bond issue, charter reform, etc.). And in a political system which is relatively amorphous, such leadership can also run the risk of being unconnected to public opinion. The lack of organized groups and parties deprives the mayor not only of potential support; it deprives him of information as well. As a result, mayoral leadership can be sharply divorced from the policy preferences of relevant constituencies. In the spring of 1970, when Mayor Reading suggested that National Guard troops be called to operate buses in the midst of a tension-filled transit strike, his proposal was sharply criticized by *both* labor and management.

It is important to recognize that hortatory leadership need not necessarily be an isolated public speaking exercise. If a political system contains active groups to whom a mayor can appeal and from whom he can gain information, then the mayor can urge those groups to help him in influencing others. Thus, the extent to which public exhortation can be used to supplement meager financial and jurisdictional resources will depend in part on the structuring of the mayor's audience.

Election campaigns can provide political actors an opportunity to appeal to the public. But if electoral competition is nonexistent, there is no incentive for groups of councilmen to join together in framing a program. If no boats are rocked, then no competition may surface. Contested elections, which can have the effect of defining alternatives and building support for those alternatives, might provide both a start and a direction for the victors.

Of course, a lack of leadership in the mayor's office does not mean that no leadership is being exerted in the urban political system. In Oakland, the city manager has certainly been a leader within the city bureaucracy. And the weakness of elected officials has doubtless helped Oakland's ghetto residents to increase their control over federal poverty programs and model cities programs.[34] But leadership exerted by elected officials differs from that of city managers and poverty program administrators in being potentially subject to popular review at regular intervals. Although the absence of groups, parties, and electoral competition can severely limit the effectiveness of that review and dilute the influence of citizens on the government, the potential for popular control is not trivial. For elections can provide a vehicle for mobilizing the resource of numbers against other resources such as money or social standing or control of communications media. The lack of power on the part of elected officials may lead to greater power for those who presently hold these other resources.

A city manager or program administrator may be responsive to citizens' preferences because he deems it to be a wise policy to act in such a manner. But under conditions of electoral competition, an elected official has a more direct incentive to pay attention to public concerns. Furthermore, there is evidence that Americans *expect* their local elected leaders to be more responsive than nonelected administrators are. As Almond and Verba have shown, Americans—unlike Germans, Italians, or Mexicans—are much more likely to contact elected political leaders than administrators if they wish to influence their local government.[35]

Because of their visibility, and the expectation that they might stimulate and respond to the concerns of urban constituencies, mayors have been a natural focus of those who study political leadership in cities. Mayors who can exert leadership by successfully pyramiding political resources are to be admired, but the ability to pyramid resources is by no means universal. Furthermore, it is often the case that people—even those in public office—try to avoid the exercise of power. Oakland officials are not unique in their avoidance behavior. Young's study of the early Washington community shows a group of men at the pinnacle of national politics who had a strong distaste for power.[36] And William K. Muir has pointed out that policemen, reacting to their hostile environment and the potential costs of decisive action, often devise ways of escaping the exercise of power.[37]

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[34] See Judith V. May, "Two Model Cities: Political Development on the Local Level" (Paper delivered at annual meeting of the American Political Science Association, New York, 1969).

[35] Gabriel Almond and Sidney Verba. *The Civic Culture* (Boston: Little, Brown and Company, 1965), p. 148.

[36] Young, *The Washington Community*.

Clearly, there is substantial variability in opportunities for leadership and in public actors' willingness to exercise it. Therefore, we must continue to search for the preconditions of leadership. How do alternative governmental structures influence the ability of officials to direct policy? What personal characteristics are most strongly related to the ability—and the desire—to pyramid resources successfully? Finally, because the opportunities for leadership appear to be closely linked to characteristics of the political system, we should try to discover those factors that encourage—or inhibit—the building of politically interested groups in cities. We also need to know more about the kinds of leadership that are possible in low resource, unstructured political arenas. Only through an analysis of these environmental and personal factors can we understand the constraints on—and potential for—urban political leadership.

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[37] William K. Muir. Jr., "The Development of Policemen" (Paper delivered at annual meeting of the American Political Science Association, Los Angeles, 1970).

Do Modern Bureaucracies Dominate Underdeveloped Polities? A Test of the Imbalance Thesis

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A number of analysts have pointed to imbalance between political and administrative institutions as a basic feature of politics in underdeveloped systems. Central to this idea of political-administrative imbalance are the observations that:

> First, in a large number of new states, representative institutional structures . . . are characterized by extreme weakness and fragility. Second, the bureaucratic sector, possessing high functional capability and high relevance to economic development, has gained an overwhelming degree of influence over government policy.[1]

According to the institutional imbalance literature, lack of correspondence between political and administrative development is anything but coincidental—for a developed bureaucratic system is itself thought to inhibit political modernization; accordingly, the imbalance argument tends to focus on the politically pathological consequences of modern bureaucracy. The purpose of this research note is to advance understanding of the relationship between administrative and political development by more closely examining a key premise of the institutional imbalance argument.

The Hypothesis

According to the imbalance thesis, the very presence of modern bureaucracy in "Third World" nations presents a formidable obstacle to political development. The tendency of administrative modernity to outstrip political development is considered to be so critical that Lucian Pye terms it "the great problem today in nation-building."[2] What makes this situation so crucial, it is held, is modern bureaucracy's tendency to become overpoliticized—a process that is reflected in its capacity to attract the most able political leaders, undermine party systems, overawe legislatures, curtail popular participation, exploit court systems, and capture interest groups.[3] It is in this sense that Ferrel Heady terms the political systems of these nations "askew," so weak are their representative structures in the face of modern administrative systems.[4] In short, as Fred Riggs argues, the vitality of bureaucracy relative to other political and governmental structures makes for a situation in which ". . . the political function tends to be appropriated, in considerable measure, by bureaucrats."[5]

In general, then, the chief components of the argument that bureaucratic development hinders the prospects of political development are the propositions that: (1) the relatively high level of bureaucratic development in new nations fosters overparticipation by the bureaucracy in the performance of governmental and political functions; and (2) such bureaucratic overparticipation stunts the growth of viable representative institutions.

In the absence of data by which the second proposition might be tested convincingly, the politically deleterious effects of bureaucratic overparticipation will not be questioned here. The intent of this note is to examine the contention—central in the imbalance argument—that bureaucratic overparticipation is the product of modern bureaucracy. More precisely, we shall investigate the hypothesis that: *In politically underdeveloped nations, there is a positive correlation between (1) level of bureaucratic development and (2) extent of bureaucratic overparticipation in governmental and political functions.* Confirmation of this hypothesis would imply that the institutional imbalance argument is essentially correct; rejection would suggest that the argument should be modified or abandoned.

Procedures

As students of administration are all too keenly aware, even the most basic cross-national data on bureaucracy are generally unavailable. Given this difficulty, systematic hypothesis testing remains a relative rarity in

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[1] Michael F. Lofchie, "Representative Government, Bureaucracy, and Political Development: The African Case," *Journal of Developing Areas*, 2 (October, 1967). pp. 39–40.

[2] Lucian W. Pye, *Aspects of Political Development* (Boston: Little, Brown, 1966), p. 19.

[3] Fred W. Riggs, "Bureaucrats and Political Development: A Paradoxical View," in *Bureaucracy and Political Development*, ed. Joseph LaPalombara (Princeton: Princeton University Press, 1963), pp. 120–167.

[4] Ferrel Heady. *Public Administration: A Comparative Perspective* (Englewood Cliffs: Prentice-Hall, 1966), pp. 64–65.

[5] Riggs, "Bureaucrats and Political Development," p. 120.

Table 1. Bureaucratic Development and Political Overparticipation

		BUREAUCRATIC DEVELOPMENT	
		+	−
PARTICIPATION IN POLITICAL FUNCTIONS	Not Over-participant	*24 Nations* Argentina Bolivia Brazil Ceylon Chile Colombia Cuba Dominican Republic Ecuador El Salvador Guatemala Haiti Honduras India Lebanon Liberia Libya Malaya Morocco Nicaragua Panama Paraguay Peru Venezuela	*8 Nations* Afghanistan Algeria Burma Cambodia Jordan Laos Saudi Arabia Thailand
	Over-participant	*6 Nations* Ghana Guinea Mexico Turkey Union of South Africa United Arab Republic	*19 Nations* Cameroun C.A.R. Congo Dahomey Ethiopia Gabon Iran Ivory Coast Mali Mauritania Niger Nigeria Sierra Leone Somalia Sudan Tanganyika Togo Uganda Upper Volta

$\chi^2 = 14.8$ d.f. $= 1$
$p < .001$

comparative administration; significant propositions more often seem to be accepted on faith than on convincing evidence. Fortunately, the key concepts of our hypothesis can be operationalized in terms of presently available data.

Bureaucratic Overparticipation. In order to measure bureaucratic overparticipation, one must answer both empirical (How much?) and evaluative (Too much?) questions. In the concluding chapter of *The Politics of the Developing Areas,* James Coleman designed a model system in which political and governmental functions are performed by specific structures:

> . . . rule-making primarily by parliaments and secondarily by executives; rule application by bureaucracies functioning under political executives; rule adjudication by an independent judiciary; interest articulation by associational interest groups and/or competing parties; interest aggregation by competing parties and/or parliaments; political socialization and recruitment by system-wide secondary structures that penetrate primary structures; and political communication by autonomous and differentiated media of communication.[6]

The volume's five area experts (Coleman, Lucian Pye, Myron Weiner, Dankwart Rustow, and George Blanksten) evaluated political systems in light of Coleman's model, noting instances in which specific structures deviated from the model by overparticipating in the performance of governmental or political functions. In all, seventy-six nations in Latin America, Asia, and Africa were evaluated. The two bureaucratic overparticipation variables employed here are taken directly from the judgments of these area specialists.

Bureaucratic Development. There is little agreement among experts as to what the concept of "bureaucratic development" entails. Equally pressing is the lack of hard data needed to operationalize the concept, however it is defined. But although formidable, these difficulties are not insurmountable. The Banks and Textor variable "Character of Bureaucracy," while less than an ideal measure, is an often-employed index of bureaucratic development.[7] This variable sep-

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[6] James S. Coleman, "Conclusion: The Political Systems of the Developing Areas," in *The Politics of the Developing Areas,* ed. Gabriel Almond and James Coleman (Princeton: Princeton University Press, 1960), pp. 559–560.

[7] For such a use of this variable, see, e.g., Gary L. Buck and Alvin L. Jacobson, "Social Evolution

Table 2. Bureaucratic Development and Governmental Overparticipation

		BUREAUCRATIC DEVELOPMENT	
		+	−
PARTICIPATION IN GOVERNMENTAL FUNCTIONS	Not Over-participant	*20 Nations* Bolivia Ceylon Chile Colombia Cuba Dominican Republic Ecuador Guatemala Haiti Honduras India Lebanon Liberia Libya Malaya Nicaragua Panama Paraguay Turkey Venezuela	*5 Nations* Afghanistan Algeria Cambodia Ethiopia Laos
	Over-participant	*10 Nations* Argentina Brazil El Salvador Ghana Guinea Mexico Morocco Peru Union of South Africa United Arab Republic	*22 Nations* Burma Cameroun C.A.R. Congo Dahomey Gabon Iran Ivory Coast Jordan Mali Mauritania Niger Nigeria Saudi Arabia Sierra Leone Somalia Sudan Tanganyika Thailand Togo Uganda Upper Volta

$\chi^2 = 13.2$ d.f. $= 1$
$p < .001$

arates national bureaucracies into four categories; since none of the systems in the present analysis falls into the "modern" class, the relevant categories are:

(1) *semi-modern*—"largely 'rationalized' bureaucratic structure of limited efficiency because of shortage of skilled personnel, inadequacy of recruitment or performance criteria, excessive intrusion by non-administrative organs, or partially non-congruent social institutions."

(2) *post-colonial transitional*—"largely rationalized ex-colonial bureaucratic structure in process of personnel 'nationalization' and adaptation to servicing or restructuring of autochthonous social institutions."

(3) *traditional*—"largely non-rationalized bureaucratic structure performing in the context of an ascriptive or deferential stratification system."[8]

For purposes of analysis, Banks and Textor's procedure of dichotomizing this variable between the "semi-modern" and "post-colonial transitional" categories will be retained.

Politically Underdeveloped Nations. A procedure for gauging the degree of political development is necessitated by the fact that our hypothesis applies only to politically underdeveloped nations. On the basis of indices of stability, legitimacy, and participation, the author elsewhere devised an instrument for dichotomizing nations into categories of politically "developed" or "underdeveloped."[9] Operation-

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and Structural-Functional Analysis: An Empirical Test," *American Sociological Review*, 33 (June, 1968), 343–355; D. Stanley Eitzen, "The Use of Banks and Textor's 'A Cross-Polity Survey' for the Ranking of Nations: A Methodological Note," *Social and Economic Studies*, 16 (September, 1967), 326–329; John Forward, "Toward an Empirical Framework for Ecological Studies in Comparative Public Administration," in *Readings in Comparative Public Administration*, ed. Nimrod Raphaeli (Boston: Allyn and Bacon, 1967), pp. 450–472; and Andreas C. Tsantis, "Political Factors in Economic Development," *Comparative Politics*, 2 (October, 1969), 63–78.

[8] Arthur S. Banks and Robert B. Textor, *A Cross-Polity Survey* (Cambridge: M.I.T. Press, 1963), pp. 112–113.

[9] Lee Sigelman, "Modernization and the Political System: A Critique and Preliminary Empirical Analysis," *Sage Professional Papers in Comparative Politics*, Vol. 2, Series No. 01-016 (1971).

ally, then, formation of a sample of nations proceeded as follows: A list of nations common to the Coleman and Banks-Textor analyses was drawn up. From this list, five nations (Israel, Costa Rica, Philippines, Tunisia, and Uruguay) were eliminated because they were considered politically "developed." This left fifty-seven Latin American, Asian, and African nations for a pool of politically underdeveloped nations.

Findings and Implications

Tables 1 and 2 strongly confirm the hypothesis that bureaucratic development and overparticipation are interrelated in politically underdeveloped nations; the chi-square values for each matrix are significant beyond the .001 level.

If the imbalance argument is substantially correct, a disproportionate number of cases should be clustered in the lower-left and upper-right cells of each matrix. Inspection reveals, however, that *the cases are arranged in precisely the opposite pattern from that expected on the basis of the imbalance thesis.* The bulk of the relatively developed bureaucracies do *not* overparticipate: only six of the thirty developed bureaucracies overparticipate in political functions, only ten of thirty overparticipate in governmental functions. On the other hand, the vast majority of the underdeveloped bureaucracies *do* overparticipate: nineteen of the twenty-seven underdeveloped bureaucracies overparticipate in political functions, and fully twenty-two of twenty-seven in governmental functions. More generally, comparison of expected and obtained frequencies for each table reveals that the significant chi-square values are products of an *inverse* relationship between bureaucratic development and overparticipation.

In interpreting these findings, it should be borne in mind that the data upon which they are based are judgmental in character. But if this analysis has any merit, it suggests that part of the accepted wisdom of comparative administration is badly in need of rethinking: no longer can it be argued on overparticipation grounds that bureaucratic development is politically dysfunctional. If modern bureaucracy is thought to hinder political development, then some alternative grounds for such hindrance must be specified. More positively, if bureaucratic overparticipation is conceded to hamper the chances of political development, the present findings suggest that modern bureaucracy may actually facilitate political development—for the relatively modern bureaucracies seldom overparticipate.[10] This last point is directly in keeping with the view—for which the author has presented some preliminary evidence elsewhere[11]—that the presence of a relatively modern national administrative system is a necessary precondition of, not a hindrance to, societal modernization, including political development.

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[10] Alternatively, according to a recent paper by Fred Riggs, "We may conclude . . . that a dominant bureaucracy will necessarily sacrifice administrative to political considerations, thereby impairing administrative performance." See Riggs's "Administrative Reform and Political Responsiveness: A Theory of Dynamic Balancing," *Sage Professional Papers in Comparative Politics,* Vol. 1, Series No. 01-010 (1970), p. 579. This interpretation. which is certainly plausible, reverses the causal sequence of the hypothesis tested here, and in the process renders Riggs virtually immune to disconfirmation by cross-sectional data; combining the hypothesis tested here with the later Riggs interpretation, bureaucratic overparticipation should be simultaneously correlated with both bureaucratic development and bureaucratic decay.

[11] Sigelman, "Modernization and the Political System . . ."

The Permanent Court of International Justice, the International Court of Justice, the League of Nations, and the United Nations: A Comparative Empirical Survey*

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The scholarly literature relevant to the PCIJ, ICJ, League and UN contains a number of complex dimensions.[1] Until recently, the literature has been primarily normative, concerned with prescriptive questions such as how international institutions might be designed to minimize violence. Both optimism and pessimism about the role of international institutions have been voiced during the past fifty years, with pessimism predominating over the long run.[2] Although the evaluative orientation continues to play a large role in the contemporary literature, the systematic development of concepts and testing of propositions has begun in recent years.[3] Scholars have been increasingly interested in investigating systematically the actual as contrasted with the hoped-for role of international institutions, not because they consider prescriptive questions unimportant but because they wish to base their evaluative discussions on solid empirical grounds. In the long run, this methodological trend in the literature may dampen the optimism-pessimism variations so that more balanced theoretical positions can be developed.

These beginnings of empirical research have been noted by Alger[4] and have become somewhat more apparent in recent issues of periodicals such as *International Organization,* but we still lack empirically tested or even testable theory about the role of international law and organization in the contemporary world. The purpose of this article is *not* to present empirically tested theory but rather to provide a mapping of some empirical relationships involving the four institutions. In other writings, both authors have applied some of the data presented in this article to a theoretical framework.[5] In

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[1] For a general overview of the literature on international law, see William D. Coplin, "Current Studies of the Functions of International Law: Assessments and Suggestions," in *Political Science Annual,* II, ed. James A. Robinson (New York: Bobbs-Merrill, 1970), pp. 149–208. The literature on international organization is surveyed in Robert E. Riggs, Karen F. Hanson, Mary Heinz, Barry B. Hughes, and Thomas J. Volgy, "Behavioralism in the Study of the United Nations," *World Politics,* 22 (January, 1970), 197–237; and Chadwick F. Alger, "Research on Research: A Decade of Quantitative and Field Research on International Organizations," *International Organization,* 24 (Summer, 1970), 414–450.

[2] For example, compare F. P. Walter, *A History of the League of Nations* (New York: Oxford University Press, 1952) to Gerhard Niemeyer, "The Balance Sheet of the League Experience," *International Organization,* 6 (1952), 537–558; or, the various writers discussed in Inis L. Claude, Jr., *Power and International Relations* (New York: Random House, 1962); or, Morton A. Kaplan and Nicholas deB. Katzenbach, *The Political Foundations of International Law* (New York: John Wiley & Sons, 1961) to C. Wilfred Jenks, *The Common Law of Mankind* (New York: Praeger, 1958).

[3] Many of the more systematic studies of the four institutions are discussed in Alger as well as Riggs et al. Those of particular relevance to this study, however, are Quincy Wright, *The Study of War,* 2nd ed. (Chicago: University of Chicago Press, 1965), pp. 1043–49; K. J. Holsti, "Resolving International Conflicts: A Taxonomy of Behavior and Some Figures on Procedure," *Journal of Conflict Resolution,* 10 (September, 1966), 272–297; William D. Coplin. "The World Court in the International Bargaining Process," in *The United Nations System and Its Functions,* ed. Robert W. Gregg and Michael Barkun (Princeton: D. Van Nostrand, 1968), pp. 317–333; and Ernst B. Haas, *Collective Security and the Future International System* (Denver: University of Denver. 1968). Most systematic studies of the World Court have focused on the question of influences on the voting behavior of the Judges. For example, see Thomas Hensley, "National Bias and the International Court of Justice," *Midwest Journal of Political Science,* 12 (November, 1968), 568–586. Systematic studies of the role of the United Nations in international conflict similar to the Haas study are beginning to appear. For example, see Thomas Hensley, "Conditions Related to the Conflict Management Activities of the United Nations," Diss. Iowa 1970; and James E. Harf, "Inter-Nation Conflict Resolution: Development and Test of a Causal Model," paper delivered at the annual meeting of the Midwest Political Science Association, 1971. (revised mimeo).

[4] Alger, "Research on Research . . ."

[5] The approach that examines the impact of international institutions on the international bargaining process is outlined in Coplin "Current Studies of the Functions of International Law," in Robinson, *Political Science Annual,* pp. 178–188. Rochester's dissertation (presently being completed), "The International System as a Prismatic Polity: Assessing Political Development Through a Comparative Em-

this article, we merely wish to present some empirical relationships that highlight similarities and differences among the four institutions that scholars might find useful in their empirical and theoretical pursuits. At a methodological level, we wish to demonstrate the usefulness of data collected across cases involving international institutions. Ultimately, we hope to contribute to empirical theory concerning the role of international organizations in bargaining relationships among states.

With those aims in mind, we first outline the basic dimensions of the data collected on the four international institutions. We then present selected findings and their theoretical implications, focusing primarily on whether structural differences (PCIJ and ICJ versus the League and UN) or historical differences (PCIJ and League versus ICJ and UN) account for the patterns in the data. A final section briefly discusses an analytical framework which helps organize the data presented in the article and which, more generally, maps suggested research strategies to be used in investigating the role of international institutions in the international bargaining process.

Description of the Data

The data used in this paper and described below includes all disputes (1) which occurred between 1920 and 1968, (2) which directly involved only two states, and (3) which were considered in at least one of the four institutions. Twenty-four cases were identified for the PCIJ, thirty-one for the ICJ, thirty-five for the League, and thirty-one for the UN.[6] (See Appendix A-D)

The decision to focus on two-party or dyadic disputes was made primarily for practical reasons, to avoid the complexity that would result if such cases as the series of Berlin crises or the Middle East confrontations were part of the data set. At the same time, the desire to compare the two Courts on the one hand and the League and UN on the other required the selection of two-party cases since dyadic cases outnumbered multilateral cases for the two Courts by fifty-five to five.

Table 1 lists the variables in each dispute which are used in this paper.[7] The variables are listed in one of three classes: (1) those that focus on the characteristics of individual participants, such as the amount of participation or the level of economic development; (2) those that focus on the relationship between the states in the dyad, such as economic interdependence or political similarity; and (3) those that focus on the characteristics of the case. The specific definitions of each of these variables will be discussed in the course of the paper.

The Use of Statistics in the Present Analysis

The methodological rule of thumb applied by the authors throughout the paper is to employ the least complicated mode of presentation and statistical analysis possible, given the descriptive and theoretical purposes at hand. Comparison of the four institutions will be made by examining the frequency distributions on different variables for the four institutions. Tables containing these distributions will be

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pirical Analysis of the Role of the World Court. League of Nations and United Nations in Dyadic Disputes," employs the data in a systematic analysis of political development in the international system over the past fifty years. The analysis is based on a theoretical model constructed by synthesizing various developmental models found in the comparative politics literature, and represents an attempt to examine empirically some questions raised by (among others) Chadwick F. Alger in "Comparison of Intranational and International Politics," *American Political Science Review*, 57 (June, 1963), 406–419.

[6] With regard to the Court, only contentious disputes are included in this study; cases involving advisory opinions are omitted. Lists of cases can be found in Wright, *Study of War*, p. 1430; Haas. *Collective Security*, pp 46–47; and Holsti, "Resolving International Conflicts . . . ," 293–296. More detailed but relatively brief information on the cases can be found in the *World Court Reports* and *The I.C.J. Reports* for the two Courts; Walter, *History of the League*, for the League; and *Synopses of United Nations Cases in the Field of Peace and Security, 1946–65* (New York: Carnegie Endowment for International Peace, 1966), for the United Nations. Empirical data have been collected and analyzed by Ernst Haas and John Ruggie, University of California, Berkeley; Hayward R. Alker, Jr., Massachusetts Institute of Technology; James E. Harf, Ohio State University; and Thomas Hensley, Kent State University.

[7] Space permits us to focus on only a few variables in this paper. Information on these selected variables as well as the complete list of variables employed in our larger data-gathering project is available from the authors upon request. Inquiries should be sent to William D. Coplin, International Relations Program, Syracuse University, Syracuse. New York 13210. The complete data set has been created in machine-readable form and can be obtained from the International Relations Archive of the Inter-University Consortium for Political Research. For those scholars who are also teachers, the data along with a discussion of how analysis in the study of international law and organization in the international bargaining process can be performed is available in inexpensive form. (*Learning Package #3: Dyadic [Two-Party] Disputes Before the World Courts, League and UN: Empirical Materials.* [Chicago: Markham Publishing Company, 1971].)

Table 1. Variables Employed in the Analysis of Two-Party Disputes

I. Participant Characteristics
  1. Number of Participations Per State For Each Institution
     a. One, b. Two, c. Three, d. Four, e. Five, f. Six, g. Over six
  2. Nature of the Political System
     a. Closed, b. Semi-open, c. Open
  3. Economic Development
     a. Underdeveloped, b. Intermediate, c. Developed
  4. Geocultural Region
     a. Western Community, b. Latin America, c. Eastern Europe, d. Other

II. Relationships Between Participants
  1. Political
     a. Same, b. Different
  2. Economic
     a. Same, b. Different
  3. Geocultural
     a. Same, b. Different
  4. Military Distance
     a. Direct allies, b. No military ties, c. Members of opposing alliances
  5. Economic Interdependence
     a. Heavy, b. Moderate, c. None
  6. Geographical Proximity
     a. Common border, b. One to five hundred miles between borders, c. Over five hundred miles between borders
  7. Relative GNP Rankings Between Participants in Unilaterally Initiated Cases
     a. Same rank, b. Initiator bigger, c. Initiator smaller

III. Characteristics of the Case
  1. Mode of Submission
     a. Joint, b. Nonjoint
  2. Hostilities Involved
     a. No hostilities, b. Hostilities
  3. Type of Issue
     a. Contractual, b. Treatment of persons, c. Territorial, d. Threat of hostilities, e. Outbreak of hostilities
  4. Number of Months Between Submission of Dispute and Disposal by the Institution
     a. One to twelve months, b. Over twelve months
  5. Significance of Action Taken by the Institution
     a. Action with consequences, b. No action taken, or action without consequences
  6. Outcome of the Dispute
     a. Settled by or with the help of the institution, b. Unsettled or settled outside the institution

presented and discussed in the body of the paper.

For each variable discussed (e.g., type of submission), we are interested in determining: (1) the relative significance of historical (that is, PCIJ/League vs. ICJ/UN) as opposed to structural (that is, Court vs. League/UN) factors in accounting for the frequency distribution pattern for the four institutions, and (2) the degree of difference between pairs of institutions (such as PCIJ vs. ICJ and League vs. UN). Statistical measures will be used to summarize the degree of difference between institutions on a given variable.[8] Regarding the first concern, we will derive a summary measure of the relative impact of historical as opposed to structural factors on each variable by aggregating the frequency distributions for the four institutions in the following manner:

	Variable (Type of submission)	
	Joint	Nonjoint
PCIJ and League		
ICJ and UN		

(Matrix A)

	Variable (Type of submission)	
	Joint	Nonjoint
PCIJ and ICJ		
League and UN		

(Matrix B)

The PCIJ and League vs. ICJ and UN matrix tells us the impact of the *historical period* while the PCIJ and ICJ vs. League and UN matrix tells us the impact of *institutional structure*. An index produced by the formula

$$\frac{\text{Measure of Association for Matrix A}}{\text{Measure of Association for Matrix B}}$$

will indicate historical effects over structural effects such that a quotient greater than one

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[8] The variables on which data were gathered are either nominal or ordinal. Our statistic for dichotomous nominal variables is phi; for non-dichotomous nominal variables, lambda. Wilcoxon's signed-ranks test is used for the ordinal variables. See Linton C. Freeman, *Elementary Applied Statistics* (New York: John Wiley, 1965). All three of these statistics are commonly used as measures of association and are employed here as shorthand measures of the degree of difference between the distributions for the different institutions.

Table 2. Frequency Distribution of Participation in PCIJ, ICJ, League, and UN for Each State (Percentage of States Using the Institutions)†

	Usage per State						
	1	2	3	4	5	6	More than 6
PCIJ *N=21	43	29	5	14	0	10	0
ICJ *N=32	72	13	3	3	0	3	6
League *N=29	39	25	18	7	0	11	0
UN *N=41	71	15	7	7	0	0	0

* Number of different states employing the institution.

† The usage per state variable was treated as an ordinal measure and the institutions as nominal categories. As a result, Wilcoxon's signed-ranks test was employed. The matrix values are:

	ICJ	League
PCIJ	.27	.06
UN	.01	.36

(Historical/Structural Index=.32/.02=16.0)

This indicates that historical period was far more important than the structural factor in affecting the differences between the institutions.

would suggest that historical period is more responsible for the frequency distribution pattern, and a quotient less than one would suggest that the pattern is more attributable to structural effects.

As for the second concern, statistical measures will be used to summarize the degree of difference on a given variable for each of the following pairs of institutions: PCIJ-ICJ, PCIJ-League, League-UN, and ICJ-UN. The summarizing statistics for these four comparisons on each variable will be presented in the following matrix format, with the value in cell A representing the summary measure for the PCIJ-ICJ comparison, the value in cell B representing the summary measure for the PCIJ-League comparison, etc.:

	ICJ	League
PCIJ	A	B
UN	C	D

All of the summarizing statistics (the Historical/Structural Index, as well as the summary measures of the four comparisons) appear as footnotes to the tables.

Participant Behavior and Characteristics

Because the state has traditionally been considered the primary actor in the international bargaining process, we find it plausible to begin our analysis of the role of the PCIJ, ICJ, League and UN in the international bargaining process by studying the relationship between the characteristics of states and their behavior toward the four institutions when they are involved in two-party disputes. We will attempt to answer the following questions: (1) How frequently have states used these institutions? (2) In what ways are the institutions brought into the bargaining process in the first place? (3) How do domestic political characteristics, level of economic development, and geocultural attributes relate to the usage of the four institutions by states?

Frequency of Usage. In general, few states have made frequent use of any of the four institutions. Table 2 indicates that with two exceptions, no state used any of the institutions more than six times, and the majority of states used the institutions less than three times. One major difference in the pattern of usage between the PCIJ and League on the one hand, and the ICJ and UN on the other is that a larger number of states have used the post-World War II institutions only once.

One interpretation of this finding is that there appears to be a stronger negative learning effect in the postwar than the interwar period.[9]

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[9] To test the nature of "learning" among users of the institutions would require more microanalysis than is possible given the purposes of this paper. For a discussion of learning effects at the international system level see Hayward R. Alker, Jr., "On the Regulation of International Disputes: Toward a Stochastic Model of Directive Behavior," paper delivered at the 1969 APSA Annual Convention (mimeo).

Assessing the costs against the benefits, states have only been willing to take disputes to the ICJ or UN once. Both institutions have produced few "winners" in any dispute, because they have reached primarily compromise decisions; hence there is little gain to be expected from using the institutions. At the same time, domestic political elements within states can be expected to react negatively to the use of the institutions unless there is a clearcut victory.

Mode of Introduction. Let us now consider the ways in which the institutions first become involved in the international bargaining process. Although the mode of introduction between the two Courts on the one hand and the League and UN on the other differ substantially,[10] the key distinction is between submisssion through joint agreement and submission by one of the primary parties.[11] Joint submission indicates some willingness on the part of *both* parties to resolve the issue or at least to give the international institutions a chance. In contrast, action by one of the parties with or without a third party indicates that the initiating side is attempting to enlist the institution in a fight against the other side. Lynn H. Miller, for example, argues in a case study of the Kashmir Dispute that when India "finally took the issue to the international forum, they went seeking a collective action against Pakistan rather than a neutral agency that would separate the contestants."[12]

Table 3 presents the cases broken down into joint submission and nonjoint submission for the four institutions. Given the structural differences of the institutions, one would expect a greater tendency for joint submissions to the Court than the League or UN. However, while there is some structural effect in evidence, joint submissions on the whole were much more a part of the interwar period than the postwar period.[13] States used both the PCIJ and League moreso than the ICJ and UN as an independent third-party rather than as a political institution to enlist on its side. Both historical period and institutional structure appear to have had an impact on the mode of introduction.

Table 3. Distribution of Cases According to Method of Introduction for PCIJ, ICJ, League, and UN (Percentages)†

	Joint Submission	Nonjoint Submission
PCIJ N=24	42	58
ICJ N=31	10	90
League N=32*	25	75
UN N=31	3	97

* Three League cases were omitted since they involved third-party introduction.

† Since both the joint/nonjoint variable and the institutions are nominal categories, phi values were computed as a measure of difference between the institutions. The matrix values were:

	ICJ	League	
PCIJ	.37	.18	(Historical/Structural Index
UN	.13	.31	=.25/.10=2.5)

Attributes of the Participants. In our analysis we have employed two variables to measure participation. The first is merely the frequency of use by each state, while the second is a weighted frequency. States were given a "3" for each initiated participation, a "2" for each participation in which they submitted a dispute jointly, a "1" for each participation in which they responded to a unilateral initiation by another state, and a "0" when they completely ignored a unilateral submission directed at them.[14]

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[10] The original coding categories for the PCIJ and ICJ were: (1) Mutual submission, (2) Compulsory jurisdiction accepted by respondent, (3) Compulsory jurisdiction challenged by respondent but overruled by the Court, (4) Compulsory jurisdiction challenged by the respondent and upheld by the Court, (5) Application accepted by the second party and (6) Application refused by the second party. For the League and UN, the categories were (1) Joint submission, (2) Unilateral submission by one participant. (3) Unilateral submission by third party. (4) Multilateral by third parties without participants, (5) Multilateral by third parties with one of the participants and (6) Multilateral by third party with both participants.

[11] The sixfold categories were collapsed into dichotomies by including the first category for Court and League-UN as well as the sixth for League-UN in the joint category, and excluding categories three, four, and five for the League-UN. All of the rest were classified as nonjoint submissions.

[12] Lynn H. Miller, "The Kashmir Dispute," in *International Law and Political Crisis: An Analytic Casebook*, ed. Lawrence Scheinman and David Wilkinson (Boston: Little, Brown, 1968), p. 49.

[13] The evidence for the sharp difference between the PCIJ and ICJ is even more substantial if one counts the four unilateral applications that were *immediately accepted by the respondent parties* in the interwar period as joint submissions. There were none of that type in the ICJ period.

[14] The "0" weighting was applied to those states that refused to respond to a unilateral application in the World Court. In addition, the four cases in the PCIJ period in which the responding states accepted application were weighted as if they were joint submissions. This weighting decision was made because it appears that there had been previous tacit if not overt agreement between both states.

Table 4. Raw and Weighted Frequency Distributions of Competitiveness of Political Systems for Participation in the PCIJ, ICJ, League, and UN (Percentages)†

	Closed	Semi-Open	Open
PCIJ			
Raw N=48	29	21	50
Weighted N=96	23	21	56
ICJ			
Raw N=62	26	14	60
Weighted N=116	13	12	75
League			
Raw N=70	31	23	46
Weighted N=132	30	24	46
UN			
Raw N=62	37	29	34
Weighted N=120	38	30	32

† The open-closed variable was treated as an ordinal measure and the institutions as nominal. Hence, Wilcoxon's signed-ranks test was employed to measure difference. The non-weighted matrix is:

	ICJ	League	
PCIJ	.09	.05	(Historical/Structural Index
UN	.24	.12	=.01/.15=.07)

The weighted matrix is:

PCIJ	.22	.09	(Historical/Structural Index
UN	.44	.13	=.05/.27=.19)

Given these indices of participation as the dependent variable, we ask what type of state is most likely to use one of these institutions. Three of the national attributes more commonly cited in the literature are: (1) the nature of the political system (2) the stage of economic development, and (3) the sociocultural background of the state.[15] Using the raw and weighted frequencies, we plot the distribution of each of these variables for the four institutions.

One of the dimensions most frequently employed to characterize entire political systems is the open-closed continuum. We have used the competitiveness among political parties within a state as an indicator of the degree of openness of a political system, according to Fred R. von der Mehden's framework for analyzing party systems.[16] Noncompetitive party system such as the Soviet Union's were classified a "closed" political systems. Semi-competitiv party systems like South Africa's were classifie as "mixed," while competitive party systems lik that of the United States were classified a "open." Given the assumptions of some scholar that open political systems are more subject t the influence of so-called "world public opinion' and are more likely to seek public forms of international diplomacy,[17] the competitiveness o the party system seemed to be a satisfactor way to tap those aspects of the political syste most closely related to participation in international institutions.

Table 4 indicates the raw and weighted frequency distributions of the use of the four institutions by each of the three types of politica systems. There does appear to be a generall positive relationship between the openness of political system and its utilization of the fou institutions. But this relationship is not merel a simple proclivity for open political systems t employ all public forms of diplomacy. Rather, the Court is more likely to be used by open political systems than is the League or UN. Furthermore, this difference becomes stronger a one moves from the interwar to the postwar period. The weighted frequencies show an even more pronounced tendency for the open systems to resort more heavily to the Court. Since the weighting makes the willingness of the nations to initiate the use of the institutions more important in the overall frequencies, we can conclude that open systems tend not only to employ the Court more often than the League or UN but also to do the initiating.

The state's economic position is often viewed as a relevant variable in the usage of international institutions. In particular, the World Court is frequently cited as being too expensive for use by the poorer nations.[18] Using Rostow's concept of the stages of economic growth, we have discriminated three categories: (1) Underdeveloped (traditional + pre-take-off), (2) Intermediate (take-off) and (3) Developed (maturity + high mass consumption).[19] Participants have been assigned into one of the three categories, with weighted as well as raw frequency distributions determined for the four institutions.

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[15] A particularly popular argument is that international institutions serve the Western developed states. In terms of the comparative study of foreign policy, participation in international institutions might be viewed as a foreign policy behavior "determined" by such variable clusters as Rosenau's idiosyncratic, societal, role, governmental, and systemic variables. See James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics,* ed. R. Barry Farrell (Evanston: Northwestern University Press, 1966), pp. 27–93.

[16] Fred R. von der Mehden, *Politics of the Developing Nations* (Englewood Cliffs: Prentice-Hall, 1964), pp. 53–64. For an extensive discussion of the open-closed continuum applied to political systems, see William D. Coplin, *Introduction to International Politics* (Chicago: Markham Publishing Co., 1971), Chap. 3.

[17] This kind of assumption is more typical of "Wilsonian type thinking" (see Robert E. Osgood, *Ideals and Self-Interest in America's Foreign Relations* [Chicago: University of Chicago Press, 1953], pp. 172–195), based on the now discredited view that democracies are somehow morally superior to autocracies and that "world public opinion" is alive and well. Nevertheless, there is still reason to assume that political systems in which foreign policy issues are openly debated are more prone to employ international institutions if only to appease certain political factions within the country. For a list of other variables that measure political systems see Arthur S. Banks and Robert B. Textor, *The Cross-Polity Survey* (Cambridge: MIT Press, 1963).

The results, presented in Table 5, indicate that institutional structure is the most important factor in accounting for differences among the institutions. More than 60 per cent of the PCIJ and ICJ cases involve participation by developed states while less than 40 per cent of the cases in the League and UN involve developed states. The difference is accentuated even more in the weighted frequency, where the evidence indicates that the developed states initiated more often in the Court while the underdeveloped states initiated more often in the League and UN. Once again, differences in the usage of the institutions are more pronounced over time, underdeveloped states using nonjudicial institutions increasingly in the postwar era.

A final national attribute often assumed to be important in shaping the attitudes and behavior of states toward international institutions might be termed geocultural region. The variable recognizes both the geographical and cultural factors that scholars like Adda B. Bozeman consider important in understanding the growth and development of international institutions.[20] Bruce M. Russett has developed the concept of the sociocultural region[21] which corresponds closely to those geocultural factors usually thought to be most relevant to the attitudes of states toward international institutions. From Russett's findings we have classified states as members of (1) the Western Community, (2) Latin America, (3) Eastern Europe and (4) the remaining states.[22]

Table 5. Raw and Weighted Frequency Distributions of Economic Development for Participation in the PCIJ, ICJ, League, and UN (Percentages)†

	Underdeveloped	Intermediate	Developed
PCIJ			
Raw N=48	6	31	63
Weighted N=96	5	29	66
ICJ			
Raw N=62	8	19	73
Weighted N=116	7	18	75
League			
Raw N=70	16	47	37
Weighted N=132	17	49	34
UN			
Raw N=62	29	35	36
Weighted N=120	37	33	30

† The economic development variable was treated as an ordinal measure and the institutions as nominal. Hence, Wilcoxon's signed-ranks test was again employed to measure difference.

The non-weighted matrix is:

	ICJ	League	
PCIJ	.09	.28	(Historical/Structural Index
UN	.40	.10	=.03/.35=.09)

The weighted matrix is:

PCIJ	.09	.35	(Historical/Structural Index
UN	.50	.17	=.01/.43=.02)

Table 6 indicates a substantial difference between the PCIJ and ICJ on the one hand and the League and UN on the other. A majority of the users of the Court are Western countries in both historical eras. The weighted frequencies indicate that an even larger proportion of Western states are the initators. In contrast, there is more even spread among the users of the League and UN. Historical era, however, also appears to be important for the two international organizations, since there is a shift in the postwar period away from use by Eastern European and Western Community states toward higher use by the states of Asia and Africa. The findings illustrate an impact of both

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[18] For example, see Kaplan and Katzenbach, *Political Foundations*, p. 281.

[19] Post-World War II data was obtained from Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, and Harold Lasswell, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), pp. 294–298. Pre-World War II status was based on the postwar standings in addition to economic reports in the *Statesman's Yearbook*.

[20] Adda B. Bozeman, *Politics and Culture in International History* (Princeton: Princeton University Press, 1960). Also see William D. Coplin, *The Functions of International Law* (Chicago: Rand McNally, 1966), Chap. 5.

[21] Bruce M. Russett, *International Regions and the International System: A Study in Political Ecology* (Chicago: Rand McNally, 1967), pp. 14–33. The Russett groupings include both geographical and sociocultural considerations. Although a large number of classifications can be predicted by geography, a few cannot (e.g., Japan is in the Western group).

[22] Russett, pp. 24–25. Russett actually has six categories (Afro-Asia, Western Community, Latin America. Semi-Developed Latins, Eastern Europe, and Unclassifiable). The Semi-Developed Latins were grouped under Latin America, and the five unclassifiable states under Afro-Asia.

Table 6. Raw and Weighted Frequency Distributions of Geocultural Regions for Participation in the PCIJ, ICJ, League, and UN (Percentages)†

	Western Community	Latin America	Eastern Europe	Other
PCIJ				
Raw N=48	61	4	29	6
Weighted N=96	68	4	23	5
ICJ				
Raw N=62	58	13	16	13
Weighted N=116	75	11	4	10
League				
Raw N=70	40	11	33	16
Weighted N=132	39	12	30	19
UN				
Raw N=62	27	13	10	50
Weighted N=120	23	13	8	56

† The geocultural region variable as well as the institutions constitute nominal scales. Hence, the lambda statistic was employed to measure difference.
The non-weighted matrix is:

	ICJ	League	
PCIJ	.08	.02	(Historical/Structural Index
UN	.37	.32	=.21/.18=1.1)

The weighted matrix is:

PCIJ	.18	.14	(Historical/Structural Index
UN	.51	.35	=.22/.35=.63)

institutional structure and historical period, then, on the geocultural attributes of the states using these institutions.

National Attributes and the Legitimacy of International Institutions

One might interpret the findings on the relationship between national attributes and institutional usage in the context of the notion of *legitimacy*. Most commonly discussed in the study of developing political systems, the concept of legitimacy ought to play a more central role in the analysis of international law and organization.[23] Scholars such as Almond and Powell suggest that the source of legitimacy for political institutions can usually be found in the strength of the symbols represented by the governmental authorities and in the effectiveness of those authorities in meeting the demands of the people.[24] Using an analogy which assumes for reasons of ease of expression that when we talk about the "state" we are referring to foreign policy decision-makers and those who influence the decision-makers, one might argue that the legitimacy of international institutions will increase as: (1) the institutions are perceived to represent the symbols and values operating within the state's political culture, and (2) the institutions are perceived to be responsive to the interests of the states. If these two points are accepted, the findings on national attributes are interpretable.

The tendency for Western, open, and developed states to employ the PCIJ and ICJ suggests that both a symbolic basis and responsiveness are necessary to generate legitimacy for the Court in the international bargaining process.[25] At the symbolic level, the rule of law associated with open political systems and the Western geocultural regions serve as a basis of identity for the state and the international institution. At the responsiveness level, the general view that the Court tends to favor the status quo would lead developed states, particularly in their dealings with underdeveloped states, to perceive the Court as being responsive to their interests.[26] In addition, the tendency for "new" states—whether they be Eastern European states in the interwar period or the Asian-African states in the contemporary period—for the closed political systems and for the poorer states to avoid the Court suggests that neither the symbolic nor the responsive basis exists for the participation of a large part of the international political system in the operation of the Court.

Similarly, the finding that non-Western, closed, and underdeveloped states have been more willing to employ both the League and the UN suggests that a symbolic basis may not be as necessary for the legitimacy of these institutions as an image of responsiveness. Scholars have generally noted that the League and UN are better equipped to make decisions changing the status quo than is either the PCIJ or ICJ.[27]

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[23] Although Seymour Martin Lipset, in *Political Man* (New York: Doubleday, 1963), pp. 64–70, tries to distinguish between legitimacy and effectiveness, learning theory suggests that the former is generated by the latter. See Richard M. Merelman, "Learning and Legitimacy," *The American Political Science Review*, 60 (September, 1966), 548–561.

[24] Gabriel A. Almond and G. Bingham Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966), pp. 199–203.

[25] More than 50 per cent of the states that used the Courts had open political systems, had developed economies, and belonged to the Western Community.

[26] Kaplan and Katzenbach, *Political Foundations*, pp. 265–283.

[27] The assumption that the Courts support the status quo and the League and UN are more open to peaceful change is at the basis of the political-legal distinction and is common in the international

At the same time, the League and UN are probably viewed by the states themselves as being more effective than the PCIJ and ICJ in controlling to some degree the use of force by the strong against the weak. Both of these factors may play some role in creating an image of responsiveness to the interests of the underdeveloped states and hence in providing legitimacy for the League and UN in these states.

Relationships Between Participants

In this section, the relationship between the two participants in a particular dyad and the role of the four institutions in the international bargaining process will be examined. The discussion will focus on (1) the degree of similarity as well as interdependence between the two states in each dyad, and (2) the relative "power" rankings between the two states in the dyad.

Similarity and Interdependence. Similarity between states is often considered to be an important variable in determining their interactions. Karl W. Deutsch, for example, has argued that social and political similarities contribute to greater international cooperation.[28] One might employ a wide variety of variables to assess the similarity between states.[29] The three selected for this study, based on the national attributes discussed earlier, are political, economic, and geocultural similarities.

The data in Table 7 were generated by coding states as politically similar if they shared the same category of political system, economically similar if they shared the same category of economic development, and geoculturally similar if they shared the same geocultural region. If they did not share the same category, they were coded as dissimilar. There was no attempt in this dichotomous classification to scale the states along these dimensions.

Table 7. Political, Economic and Geocultural Similarities Between the Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Political		Economic		Geocultural	
	Same	Different	Same	Different	Same	Different
PCIJ N=24	50	50	50	50	46	54
ICJ N=31	35	65	61	39	39	61
League N=35	51	49	49	51	51	49
UN N=31	35	65	23	77	35	65

† Since both similarity and institution are nominal variables, phi was employed. The matrix values for political similarity are:

	ICJ	League	
PCIJ	.15	.01	(Historical/Structural Index
UN	.00	.16	=.15/.02=7.5)

The matrix values for economic similarity are:

	ICJ	League	
PCIJ	.11	.01	(Historical/Structural Index
UN	.39	.27	=.07/.20=.35)

The matrix values for geocultural similarity are:

	ICJ	League	
PCIJ	.07	.05	(Historical/Structural Index
UN	.03	.16	=.12/.02=6.0)

The pattern of similarity within dyads is complex. The PCIJ and League generally handled as many disputes involving politically dissimilar states as politically similar states, whereas the ICJ and UN were employed most often in cases involving politically dissimilar countries. While there is little difference in the pattern of economic similarity between the PCIJ and League, the ICJ and UN show opposite tendencies. In terms of geocultural similarity, historical era appears to have had a large effect, with the postwar period showing the impact of the universalization of the state system.

Moving from the similarity between members of the dyad to their interdependence, we have selected three of the more common measures—military distance, geographical proximity, and economic interdependence as measured by international trade. These three aspects tap relatively different dimensions, but all imply a degree of what Cobb and Elder call "mutual

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law and organization literature. See, for example, *Report of a Study Group on the Peaceful Settlement of International Disputes* (London: David Davies Memorial Institute of International Studies, 1966), pp. 4–6.

[28] Karl W. Deutsch, "The Probability of International Law," in *The Relevance of International Law*, ed. Karl W. Deutsch and Stanley Hoffmann (Cambridge: Schenkman Publishing Co., 1968), p. 57.

[29] Wright's most systematic presentation of the concept of distance and the related framework of field theory can be found in *The Study of International Relations* (New York: Appleton-Century-Crofts, 1955), pp. 531–567. Also see Wright's *The Study of War*, pp. 1466–1472. For a systematic discussion of a large number of similarities as well as interdependence, see Roger W. Cobb and Charles Elder, *International Community: A Regional and Global Study* (New York: Holt, Rinehart, 1970), pp. 23–55.

Table 8. Military Distance Between the Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Direct Allies	No Military Ties	Members of Opposing Alliances
PCIJ N=24	4	79	17
ICJ N=31	22	52	26
League N=35	6	94	0
UN N=31	7	87	6

† Since military distance is treated conceptually as an ordinal variable, the Wilcoxon signed-ranks test was employed. The values in the matrix are:

	ICJ	League	
PCIJ	.07	.18	(Historical/Structural Index
UN	.04	.06	=.01/.10=.10)

relevance"[30] as well as what Russett has termed "responsiveness"[31] between states. Because mutual relevance and responsiveness can lead to both cooperation and conflict, it is difficult to postulate the degree to which interdependence would be related to the role of the four institutions in the international bargaining process (as was also the case in relating the similarity measures).

In determining the degree of military interdependence between the two states in each dyad, we attempted to include what might be termed the military distance between states. Dyads were coded in terms of whether the two states were direct allies, had no military ties, or, were members of opposing alliances.[32] As a result, this variable includes something more than military interdependence *per se*. Table 8 indicates that the patterns for the League and UN are relatively similar, with an overwhelming majority of the cases involving states with no military ties. In contrast, the PCIJ and to a larger extent the ICJ, have a moderate percentage of cases involving states that were members of opposing alliances.

If one accepts the usual assumption that the Court is concerned with "legal" issues and the League and UN with "political" issues, the findings are somewhat contrary to what one would expect. It should be remembered that the sample of cases upon which this study is based is dyadic, and that members of opposing alliances are most likely to be involved in multilateral issues before the League and UN. Although this fact would explain the low number of cases involving members of opposing alliances for the League and UN, it does not explain the relatively high number of cases involving members of opposing alliances for the PCIJ and ICJ. Hence, it is fair to interpret the evidence as indicating that the PCIJ and even moreso the ICJ have not been as apolitical as some might have hoped at the inception of the institutions. All of the ICJ cases involving members of opposing alliances can be attributed to friction between the United States or United Kingdom and the Warsaw Pact countries, and the implication from these instances is that the Court has been frequently used by parties for propagandizing or legitimizing purposes rather than with expectation of settlement.

The degree of economic interdependence was roughly approximated, based on reports of international trade in the *Statesmen's Yearbook*. Dyads were coded according to three categories of economic interdependence—heavy, moderate, and none.[33] Table 9 presents frequency distribution of dyads for each of the four institutions. The results indicate effects from both historical era (PCIJ/League vs. ICJ/UN) and institutional structure (Court vs. League/UN), with the former having the larger impact. The PCIJ and League distributions are somewhat similar while the ICJ and UN distributions differ in the same direction from the institutions of the interwar period. Economic interdependence, however, appears to be less negatively associated with ICJ involvement than it does with UN involvement.

Finally, there is the question of geographical proximity which writers have traditionally

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[30] See Cobb and Elder, *International Community*. pp. 22–132 for a discussion and operationalization of the concept of mutual relevance. Also, the book suggests a variety of approaches to measuring interdependence among states.

[31] Bruce M. Russett, *Community and Contention: Britain and America in the Twentieth Century* (Cambridge: MIT Press, 1963), pp. 26–33.

[32] The data on military alliance were taken from J. David Singer and Melvin Small, "Formal Alliances, 1815–1939: A Quantitative Description," *Journal of Peace Research*, 3. No. 1 (1966), 1–32.

[33] The following criteria were employed in classifying the dyads. The dyad was coded as having no economic interdependence if neither country was mentioned as a principal trading partner of the other in the *Statesmen's Yearbook* for the year of submission of the case. High interdependence was coded if both states were mentioned in each other's descriptions as principal trading partners, while moderate interdependence was assigned to those dyads in which only one state was mentioned in the two descriptions as a principal trading partner. The measure is exceedingly rough, but the lack of available data for the interwar period prevented the use of the "Direction of Trade" statistics now available for the postwar period.

Table 9. Economic Interdependence Between Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Heavy	Moderate	None
PCIJ N=24	38	50	12
ICJ N=31	26	29	45
League N=35	43	40	17
UN N=31	10	39	51

† The economic interdependence variable is treated as an ordinal measure and the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League
PCIJ	.31	.02
UN	.14	.48

(Historical/Structural Index =.40/.05=8.0)

Table 10. Geographical Distance between Disputants in Each PCIJ, ICJ, League, and UN Case (Percentages)†

	Common Border	1 to 500 Miles	More than 500 Miles
PCIJ N=24	41	17	42
ICJ N=31	19	20	61
League N=35	80	9	11
UN N=31	55	16	29

† Since geographical distance has been treated as an ordinal measure, the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League
PCIJ	.25	.40
UN	.40	.26

(Historical/Structural Index =.29/.42=.69)

viewed as influencing the ways states deal with each other.[34] Dyads were coded for common border and for distance (between nearest borders) either below or above 500 miles. Table 10 presents the data for the four institutions. Again, the structure of the institutions as well as the historical era appear to have an impact. The League and UN have generally dealt with cases between disputants having a common border, while the Courts have had fewer dyads in this category. The impact of historical era is indicated by the extreme positions of the League and ICJ on this variable, showing the combined effects of history and structure.

An aggregate view of the six similarity and interdependence findings suggests that it is more likely for the dyad using the institutions to be dissimilar and relatively independent than to be similar and highly interdependent. This finding contrasts with the viewpoint prevalent in the literature that high interdependence and similarity, other things being equal, are likely to contribute to the development of such regional international institutions as the European Economic Community.[35] Rather than contradicting the findings of this paper, however, the contrast suggests a basic distinction between regional functional organizations such as the EEC on the one hand, and the PCIJ, ICJ, League, and UN on the other. (The distinction elaborated below applies somewhat to global functional institutions, such as the World Health Organization, as well.)

The difference between the two types of institutions is related to the role of cooperation among the states using the organization and the structure of the bargaining process as a result of that cooperation. The regional functional organization symbolizes the realization among its members that a common set of problems exists. The bargaining process that takes place within the framework of the functional organization, therefore, is thoroughly shaped by the understanding that the decisions of the organization can benefit all of the states. Consequently, states bargain over the distribution of collective costs (e.g., financial contributions) and benefits (e.g., allocation of technical assistance).[36] In contrast, peace-promoting organizations attempt to deal with issues generated not by the perception of a common problem but by the assumption that the other party to the dispute is the cause of the problem. Bargaining takes place over private benefits between the major antagonists and consists primarily of each state's attempting to convince the other state to change its behavior.

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[34] Geography is probably the oldest and most multifaceted relationship discussed in the international relations literature. Concern for the interaction of geography and relationships between states is not just a product of a geopolitical outlook (e.g., Sypkman) but is also evidenced by students of integration (e.g., Cobb and Elder, *International Community*, pp. 84–92) and power politics (e.g., Hans Morganthau, *Politics Among Nations*, 3rd ed. [New York: Alfred Knopf, 1967], pp. 106–109).

[35] For example, Deutsch, "The Probability of International Law," in *Relevance of International Law*, p. 57; and Cobb and Elder, pp. 9–11.

[36] For a general discussion of the distinction between collective and private benefits see Mancur Olson, Jr., *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965). The distinction is applied to the study of international alliances by Philip M. Burgess and James A. Robinson, "Alliances and the Theory of Collective Action: Simulation of Coalition Processes," *Midwest Journal of Political Science*, 13 (May, 1969), 194–218.

"Power" Rankings Among States. Somewhat related to the question of similarity and interdependence is the traditional concern many scholars have had for power relations among states. Although the utility of the power concept in the study of international relations is seriously questionable,[37] its prevalence in the literature prompts our consideration of it. The preoccupation with power has origins in at least two different scholarly traditions. The first is the power politics school as exemplified by the writings of Organski and Morgenthau.[38] Generally, it is assumed that the powerful will dominate the powerless and that conflict will increase as the weaker states begin to gain equality with the stronger ones. The second tradition develops out of the sociological literature and has been applied to the study of international law by Ib Martin Jarvad.[39] Arguing that states rank themselves partially in economic and military terms and partially in other terms, Jarvad postulates that states attempt to maximize their rank. Both the power tradition and the rank tradition, then, assume that states are most concerned with maintaining or increasing their positions vis-à-vis each other, however those positions may be defined.

If the assumptions of both traditions are applied to the role of the four institutions in the international bargaining process, the weaker or lower-ranked states might be expected to employ institutions more often against the stronger or higher-ranked states. This expectation would be based on the view that the guarantee of equality before the institutions explicitly developed in the charters of the four institutions is a reality. Furthermore, given the special position accorded great powers in both the League and UN, one would expect that for smaller states, the Courts would be even more appealing than the League or UN would be.

To test these ideas about what Jarvad has called "power versus equality" for the four institutions is far beyond the scope of this paper.[40] A preliminary test has been undertaken, however, by looking at the relative power as measured by total GNP of the states in each dyad and the manner in which these states employed the four institutions. The use of GNP as an indicator of power leaves much to be desired, but for our preliminary purpose it appears a satisfactory measure since it represents wealth and population size—two of the most commonly referenced indicators of power.[41] All cases in which there was a unilateral initiator have been coded in terms of whether the GNP of the initiator was greater (by at least 25 per cent), less (by at least 25 per cent), or substantially the same as that of the responding state.[42] (Cases introduced jointly or by a third party have been ignored since it is difficult to determine the degree to which the behavior of the members of the dyad was different.) Table 11 presents the results.

In terms of the theoretical questions posed by Jarvad, the data provide a striking contrast between the PCIJ and the ICJ on the one hand and the League and UN on the other. The majority of unilaterally submitted cases in the PCIJ and ICJ are initiated by the more powerful states, while the opposite is true in the League and UN. This finding suggests that states perceive the Court as the protector of the status quo and the League as well as UN as the medium for international change.[43] The substantial difference attributed to structure sug-

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[37] It appears that neither the power nor the rank tradition as applied to international relations has confronted the fact that different sources of influence operate in (1) different time periods, (2) for different states and (3) in different issue areas. Instead, it is assumed that power is a product of military and economic capability even though the writers might argue that other factors such as willingness to make commitments are involved. For a discussion of the problems in the literature see Coplin, *Introduction to International Politics,* Chap. 4.

[38] Morgenthau, *Politics Among Nations,* and A. F. K. Organski, *World Politics,* 2nd ed. (New York: Alfred A. Knopf, 1968). pp. 338–377.

[39] Ib Martin Jarvad, "Power versus Equality: An Attempt at a Systematic Analysis of the Role and Function of the International Court of Justice, 1945–1966," paper presented at the Second Conference of International Peace Research Association (IPRA), 1967 (revised mimeo). Jarvad suggests that the sociological literature relevant to the study of rank and international politics is identified in Johan Galtung, "International Relations and International Conflicts: A Sociological Approach," *Transactions of the Sixth World Congress of Sociology,* 1 (1966), 121–161.

[40] Jarvad has applied this framework only to the ICJ, but it can be applied with equal usefulness to the other three institutions.

[41] See Bruce M. Russett, *Trends in World Politics* (New York: Macmillan, 1965) for a discussion of the use of GNP as a measure of power. Jarvad, p. 4, employs six variables, all of which are related to economic development. Again, the choice of operational indicators of rank as well as power shows the dependence on a simplistic notion of influence.

[42] Data for GNP for dyads after the Second World War were taken from the *United Nations Statistical Yearbook.* For interwar data, rough approximations were made based in part on national income data in sources such as the *Statesmen's Yearbook.* Given the theoretical question and the choice of a rather arbitrary cutoff point, however, the reliability of the data seems adequate. Little difference would have resulted from raising or lowering the cutoff point ten percentage points.

[43] This point is related to the findings reported above, indicative that a large percentage of the cases before the Courts involved the developed states.

Table 11. Relative GNP Rankings between Disputants in Unilaterally Initiated Cases Before the PCIJ, ICJ, League, and UN (Percentages)†

	Same Rank	Initiator Bigger (by at least 25%)	Initiator Smaller (by at least 25%)
PCIJ			
N=14	29	57	14
ICJ			
N=28	14	64	22
League			
N=24	25	17	58
UN			
N=30	0	20	80

† Since the GNP comparison was treated as a nominal scale, lambda was employed. The values in the matrix are:

	ICJ	League
PCIJ	.00	.29
UN	.57	.25

(Historical/Structural Index =.16/.43=.37)

gests that neither type of institution is viewed as a neutral agent, but rather is considered to be an institution through which rank is maximized. In the case of the Courts, the strong appear to be maintaining or increasing their position, while in the case of the League and UN, the weak assume that they can enhance their position.

Characteristics of the Disputes and Institutional Responses

Salience of Bargaining Issues. Turning from an analysis of the actors and their relationship to each other, we now consider the characteristics of the dispute itself, in particular the salience of bargaining issues. The traditional distinction between legal and political disputes—a distinction that appears in both the international law and international organization literature—suggests that a major dimension that we should uncover in our typology is *salience*. This conclusion is based on the growing consensus that nothing inherent in the issue makes a dispute legal rather than political but that indeed the determination is made by the actors themselves.[44] A dispute is legal if the parties choose to consider it a legal dispute. By implication, then, the decision to consider a dispute primarily in the legal dimension, particularly for the side with the weaker legal argument, occurs when the state stops attaching great importance to the issue.

Table 12. Existence of Hostilities in Cases before the PCIJ, ICJ, League, and UN (Percentages)†

	No Hostilities	Hostilities
PCIJ		
N=24	100	0
ICJ		
N=31	71	29
League		
N=35	51	49
UN		
N=31	42	58

† Since hostility categories constitute a dichotomous nominal scale, phi has been employed. The values in the matrix are:

	ICJ	League
PCIJ	.40	.53
UN	.26	.10

(Historical/Structural Index =.15/.38=.39)

In searching for a typology which focuses on the dimension of salience, we find that one of the more obvious factors is the presence of hostilities.[45] We can assume that the more salient a bargaining issue, the more likely it is that the parties will resort to violence. Table 12 indicates the frequencies of disputes that involved hostilities vs. those that did not.

A comparison of the frequency distributions in Table 12 suggests that the League and UN generally handle cases involving hostilities while the Courts do not. Cutting slightly across this impact of structure on the role of hostilities in cases is the role of historical era. For both types of institutions, the interwar period involves fewer hostilities than the postwar period does. It suggests that the disputes submitted to the postwar institutions have been of higher salience to the participants than those submitted in the interwar period.

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[44] For a clear statement on this point see *Report of a Study Group on the Peaceful Settlement of International Disputes*, pp. 4–6.

[45] Future attempts at determining the salience of an issue in this area might borrow on the work of a number of scholars in collecting data and measuring international interactions. McClelland's World Event Interaction Series Project at the University of Southern California, or Rudolph Rummel's Dimensionality of Nations Project at the University of Hawaii might provide data on the more recent cases relevant to levels of hostility surrounding the relationships of states involved in a particular dispute. In a recent paper by William D. Coplin and Michael K. O'Leary, "A Simulation Model for the Analysis and Explanation of International Interactions" (delivered at the 1971 ISA National Convention), a different way of operationalizing and in fact conceptualizing the concept of salience was presented. Briefly, that method assumes that the broader the awareness of an issue within the political system of a state, the more salient the issue.

Table 13. Type of Issue in Cases Before the PCIJ, ICJ, League, and UN (Percentages)†

	Contractual	Treatment of Persons	Territorial	Threat of Hostilities	Outbreak of Hostilities	NA
PCIJ N=24	42	50	8	0	0	0
ICJ N=31	26	16	32	0	26	0
League N=35	14	12	34	6	34	0
UN N=31	3	7	16	10	58	6

† Assuming the issue typology represents an ordinal scale, the Wilcoxon's signed-ranks test has been employed. The values in the matrix are:

	ICJ	League
PCIJ	.46	.67
UN	.53	.39

(Historical/Structural Index=.33/.54=.61)

A second typology related to the salience dimension focuses on the issue-area of a particular dispute.[46] A fivefold typology has been developed: (1) contractual obligations, (2) the treatment of people, aliens, and minorities, (3) territorial disputes, (4) threatened hostilities, and (5) actual hostilities. A salience dimension is assumed to underlie the typology. This assumption is based on the viewpoint that broader and more powerful segments of the domestic political environment become mobilized as one moves from categories 1 through 5, which in turn results in increased salience for the foreign policy decision maker and hence the national actor taken as a collectivity.[47]

Table 13 compares the frequency distributions for this typology for all four institutions. A sharp difference between the Court on the one hand, and the League and UN on the other, might be expected if one accepted the view that the Court deals with legal and therefore less salient disputes than the League and UN. In fact, this expectation is generally fulfilled. At the same time that there is a difference attributable to institutional structure, there is also one attributable to historical period. The salience of disputes increases for the ICJ over the PCIJ and for the UN over the League.

The Impact of the Institutions. The scholarly literature posits a wide variety of impacts for the four institutions on the international bargaining process. Although the categories are somewhat arbitrary, one can identify three relatively distinct roles scholars have ascribed to the four institutions in the international bargaining process: (1) environmental, (2) regulative, and (3) distributive.

International law and organization have been viewed as a medium in which bargaining among states takes place. In earlier writings, one of the authors has argued that international law may best be viewed as providing a bargaining environment for states because the law provides a set of orientations in which states relate themselves to each other.[48] Young maintains that during periods of crisis the United Nations "has some effect in setting the general atmosphere and climate of opinion in which bargaining among the principals (takes) place."[49] This view of international law and organization as performing an environmental role maintains that legal norms provide a set of concepts or symbols in which actors phrase their positions, and international institutions provide a set of routinized communication and interaction channels.[50]

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[46] The issue-area concept has been developed outside the international politics field. See Robert Dahl, *Who Governs?: Democracy and Power in an American City* (New Haven: Yale University Press, 1961). James N. Rosenau, "Pre-Theories and Theories of Foreign Policy." in Farrell, *Approaches to Comparative and International Politics*, pp. 77–93, has suggested how the concept might be applied to the study of international politics.

[47] See Coplin. *Introduction to International Politics*, Chap. 3, for a discussion of the relationship between issue-area and domestic political pressure.

[48] Coplin, "Current Studies of the Functions of International Law," in Robinson, *Political Science Annual;* and Coplin, "International Organizations in the Future International Bargaining Process," *Journal of International Affairs*, 25 (Summer, 1971), 287–301.

[49] Oran R. Young, *The Politics of Force: Bargaining During International Crisis* (Princeton: Princeton University Press, 1968), p. 169.

[50] Stanley Hoffman has criticized this view of international law and organization in his essay "International Law and the Control of Force," in *The Rele-*

The second approach sees international law and organization as regulators of the international bargaining process. This regulative view is expressed in many forms including the emphasis on peace keeping in many United Nations activities,[51] Claude's concept of "preventive diplomacy,"[52] and even the concept of collective security itself.[53] With regard to international law, for example, Falk has argued that the law serves as a "braking mechanism" in helping to prevent the automatic escalation of international conflict.[54]

The third approach views international law and organization as distributive organs in the international bargaining process. Concepts of peaceful change and the pacific settlement of disputes developed within the Covenant and UN Charter as well as by a large number of writers imply that international institutions can serve in making authoritative distributions between states for a given bargaining issue.[55] In this view, the World Court and the collective security organizations are considered to be disinterested third parties making final decisions.

The three views are not necessarily incompatible. In fact, many writers combine two or all three views in their discussions. For example, while Young sees the UN as providing an environment in which states bargain, he also sees it playing the role of third party intermediary in a regulative as well as distributive capacity.[56] Moreover, the institutions themselves play different roles in different situations. In the Berlin Blockade, the UN served primarily as an environment for Soviet-American communications, while in the Suez Crisis it played primarily a regulative role.[57]

Table 14. Length of Time for Disputes in the PCIJ, ICJ, League, and UN (Percentages)†

	1–12 Months	More than 12 Months
PCIJ N=24	33	67
ICJ N=31	32	68
League N=31*	45	55
UN N=21*	67	33

* It was not possible to determine the exact time of withdrawal of a dispute from the League and UN for a number of cases (4 for the League and 10 for the UN).

† Phi was employed in testing for historical and structural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.01	.13	(Historical/Structural Index
UN	.34	.20	= .05/.22 = .23)

A variable that might be useful in assessing the degree to which the three functions are performed is the length of time that cases remain in the institutions. Table 14 indicates that the Court usually takes longer than either the League or the UN to deal with a case. In addition to the structural factor, historical effects also seem to be important, since the UN was more likely than the League to deal with cases in a year or less.

This difference suggests some ideas about the role of these institutions in the international bargaining process. It suggests that the Courts act as distributive institutions, taking their time to arrive at a decision and depending on disputes to become less salient over time. In contrast, it suggests that the role played by the League and even to a greater extent by the UN is not distributive but environmental and regulative. Since disputes containing high salience

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vance of International Law, ed. Deutsch and Hoffmann, pp. 21–47. His criticism that it is necessary to look at international law as a coercive order even though it fails to approximate such a view is directed at those writers who view international law as part of the international environment. He does not deny that international law may be approached from such a viewpoint but rather that it must be viewed also in other terms.

[51] See, for example, the discussions of the activities of the UN in the yearly *International Conciliation* volume published on issues before the General Assembly.

[52] Inis L. Claude, Jr., *Swords into Plowshares: The Problems and Progress of International Organization*, 3rd ed. (New York: Random House, 1964), pp. 385–404.

[53] Claude, pp. 223–258.

[54] Richard A. Falk, "Confrontation Diplomacy: Indonesia's Campaign to Crush Malaysia," in *International Law and Political Crisis*, ed. Scheinman and Wilkinson, p. 162.

[55] Almost all writers impute the dispute-settling function to the League or UN or both. See, for example, Claude, *Swords Into Plowshares*, pp. 193–222, and Wolfram F. Hanrieder, "International Organizations and International Systems," *Journal of Conflict Resolution*, 10 (September, 1966). 297–313. Hanrieder cites three tasks of international organization, two of which—peaceful change and the peaceful settlement of disputes—might be considered distributive. Although this function was emphasized more strongly in the interwar period than the postwar period (see, for example, Coplin, *Functions of International Law*, ch. 3), the dispute-settling function is still emphasized today.

[56] Compare discussions of the UN in Young, *The Politics of Force*, pp. 146–171, to discussions of the UN in Young, *The Intermediaries* (Princeton: Princeton University Press, 1967).

[57] Compare Lawrence Scheinman, "The Berlin Blockade," pp. 1–41, to Wolfgang Friedmann and Lawrence A. Collins, "The Suez Canal Crisis of 1956," pp. 91–126, both in Scheinman and Wilkinson.

Table 15. Actions with External Consequences Taken by the PCIJ, ICJ, League, and UN (Percentages)†

	Action with Consequences	No Decision or Consequential Action
PCIJ N=24	67	33
ICJ N=31	26	74
League N=35	74	26
UN N=31	64	36

† Phi was employed in testing for historical and tructural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.41	.26	(Historical/Structural Index
UN	.39	.11	=.26/.26=1.0)

Table 16. Outcome of Disputes in the PCIJ, ICJ, League, and UN (Percentages)†

	Settled By or With the Help of	Unsettled or Settled Outside the Institution
PCIJ N=24	67	33
ICJ N=31	32	68
League N=35	49	51
UN N=31	19	81

† Phi was employed in testing for historical and structural effects. The values in the matrix are:

	ICJ	League	
PCIJ	.34	.06	(Historical/Structural Index
UN	.15	.32	=.31/.13=2.38)

have usually been submitted to the League and UN rather than to the Court, the impact of the League and UN has appeared to be one of keeping the peace rather than developing a distributive formula to resolve the disputes.[58]

A second consideration that one ought to take into account when considering the impact of the institutions on the international bargaining process is the ability of the institution to take actions that have some consequences. For the Court, we can define this as taking decisions (judgments) that are substantive (i.e., deciding the merits of the case) as opposed to taking decisions that are strictly procedural or taking no decisions whatsoever. For the League and UN, we might define it as action in at least one of the following forms: inquiry, mediation, cease fire, truce supervision, enforcement through boycott, police action, insertion of the Secretary-General's or his deputies' presence, formation of a committee of experts, or the provision of material support. All of these activities involve more than the simple appeal to resolve the problem.[59]

Table 15 lists the frequencies with which the four institutions have taken action that involves or does not involve some consequence as defined above. Both a historical and a structural effect appears to be operating here. Generally, the interwar institutions had a better capacity to act, although the UN has done much better than the ICJ. From a theoretical point of view, the decline of the ICJ is most crucial because it represents a loss in the capacity of international institutions to make substantive decisions in bargaining among states. It suggests further that if scholars continue to view institutions as a means for settling disputes they will be disappointed.

This pessimism is further increased if we look at the record of the four institutions in settling disputes or contributing to the settlement of disputes. Using the categories suggested by Ernst Haas, we coded disputes as "settled" if both sides refrained from arguing the point (with acquiescence to a *fait accompli* not constituting settlement).[60] Table 16 indicates the distributions for the four institutions on this variable.

Again, historical and structural effects appear to be operating in the record of the four institutions although the historical trend is much stronger than it was in the case of the preceding variable. Combining the findings for Tables 15 and 16 leads one to conclude that not only is the ICJ less capable of making substantive decisions than its predecessor, but also the United Nations is less capable of resolving disputes than the League in spite of its relatively equal capacity to take actions. This would suggest that the UN is playing a role in the contemporary world that is different from the traditional role of conflict resolution or dispute settlement.

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[58] One might even posit an efficiency rating for these institutions, based on the relationship between the length of time in which the institutions handle the dispute and the salience of the dispute. For high salience disputes, the longer the dispute remains in the institutions, the less efficient the institution. If this were true, the UN is clearly a more efficient institution than the League.

[59] These categories have been adapted from those supplied by Haas, *Collective Security*, pp. 38–39.

[60] Haas, p. 44.

Generally speaking, the findings suggest that we ought to develop criteria of efficiency for the United Nations that do not require the resolution of conflict or the settlement of disputes. There is sufficient evidence here as well as in the general record of post-World War II international politics that states can deal with each other without resolving their conflicts. The UN appears to be contributing to a general situation in contemporary international politics that finds conflicts being frozen, routinized and ultimately ignored with no resolution in the normal sense of the word.[61] If this is true, we need to develop categories that look at the capacity of international institutions to transfer, to render less important, and to freeze international conflicts.

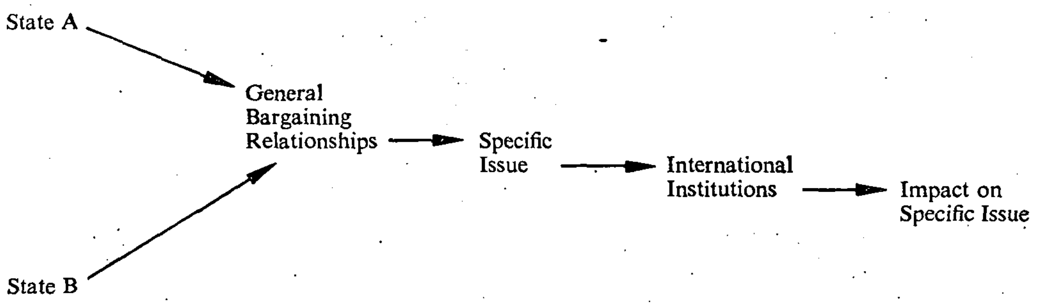
Summary and Conclusion

The purpose of this article, as stated at its beginning, has been to present a data set that allows for a comparative empirical analysis of the PCIJ, ICJ, League, and UN in the international bargaining process. Although a variety of theoretical ideas now found in the scholarly literature were related to the patterns found in the data, no attempt was made to develop a coherent theoretical model. In fact, the authors hope that this article will ultimately contribute to the development of empirically based theories relating international institutions to the international bargaining process.

As a method of summarizing what has been presented and providing some organizational guidelines for future scholarly work, Figure I presents a general framework identifying sets of variables that should be explored when examining the role of international institutions in bargaining among states. Although the scheme, and in fact the entire paper, has been focused on two-party bargaining relationships and the PCIJ, ICJ, League, and UN, it can be viewed as a general framework for studying all types of bargaining among states—including multilateral—and all types of intergovernmental organizations.

The diagram assumes that each group of variable sets operates on the one following it in the diagram and therefore indirectly on all the remaining variables. In addition, the impact of the institution on the specific bargaining issues is assumed to feed back through the foreign policy-making structures of the states and affects the entire international bargaining process. Those variable sets that are underlined have been used in one way or another in this paper.

Figure 1. Schematic Illustration of Framework Relating Variables in the Study of International Institutions in Bargaining Among States



RELEVANT VARIABLE SETS

1. *National Attributes*	1. *Similarity in Attributes*	1. *Salience*	1. *Entrance Patterns*	1. *Environmental*
2. *Previous Experience with Institution*	2. *Interdependence*	2. *Age of Dispute*	2. Processes Within Institution (e.g., consensus, voting)	2. *Regulative*
	3. *Power or Rank Relationship*	3. *Hostilities*		3. *Distributive*
	4. International Conditions (e.g., Bi-polarity)	4. Issue-Area		4. Collective-problem-solving

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[61] This point is developed in greater detail in Coplin, "International Organizations in Future International Bargaining Process."

The four diagrammed variable sets that have not been included in this study are of critical importance. Ernst Haas has shown how the conditions in the international environment relate to the role of the United Nations.[62] A number of writers including James N. Rosenau have emphasized the importance of issue area for the study of foreign policy behavior.[63] One of the authors of this article, with Michael K. O'Leary, has recently written a paper arguing for the need to include issue area in any study of international interactions.[64] The processes that take place within institutions have probably been the single most studied set of phenomena in the international organization literature.[65] Finally, the category of collective problem-solving suggests that in addition to the three general functions that are now attributed to international institutions, we ought to employ criteria that assess the degree to which international institutions contribute to the achievement of world public policy goals.[66]

Another important aspect of this scheme is the critical points it suggests for further study. We ought to be concerned with the linkages between foreign policy-making processes and the bargaining interactions among states. We should also attempt to define how general bargaining relationships lead to disputes over specific issues. Furthermore, the path of specific issues into or around international institutions should be clearly specified and studied, as should the relationships between various institutional processes and the ultimate impact the institutions have on specific issues. Finally, it is necessary to look at issue impact as a learning process, through which the foreign policy-making structures within states develop predispositions toward international institutions, including international law, that may affect their future international bargaining behavior.

Whether or not scholars accept this particular scheme is not as important as the commitment to build and empirically test theories linking international institutions to the way states behave. Until we have developed our theories and created sufficient data and analytical capabilities to distinguish the myth from the reality of the role of international institutions in the contemporary world, we are bound to be tied to the oscillating excesses of pessimism and optimism that have characterized the literature of international law and organization. More importantly, if students of international institutions are able to clarify their own thinking, ultimately they may have some useful impact on shaping public policy.

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[62] Haas, *Collective Security* and Haas, "Dynamic Environment and Static System: Revolutionary Regimes in the United Nations," in *The United Nations System*, ed. Gregg and Barkun, pp. 162–197.

[63] See Rosenau, "Pre-Theories and Theories . . . ," in *Approaches to Comparative and International Politics*, ed. Farrell.

[64] Coplin and O'Leary, "A Simulation Model . . ."

[65] See, for example, Chadwick F. Alger, "Interaction in a Committee of the United Nations General Assembly," in *Quantitative International Politics*, ed. J. David Singer (New York: Free Press, 1968), pp. 51–84; Hayward R. Alker, Jr. and Bruce M. Russett, *World Politics in the General Assembly* (New Haven: Yale University Press, 1965); John Hadwen and Johan Kaufmann, *How United Nations Decisions Are Made* (Leyden: A. W. Sythoff; 1962); Thomas Hovet, Jr., *Bloc Politics in the United Nations* (Cambridge: Harvard University Press, 1960); and Leon Gordenker, "Policy-Making and Secretariat Influence in the UN General Assembly: The Case of Public Information," in *The United Nations System*, ed. Gregg and Barkun, 136–155.

[66] See Coplin, *Introduction to International Politics*, pp. 136–155.

Appendix

A. Two-Party Cases for the Permanent Court of International Justice

	Name of Case	Participants	Date of Introduction
1	Mavrommatis Palestine Concessions	Greece/Great Britain	5/24
2	Interpretation of Article 179, Annex, Paragraph 4, of the Treaty of Neuilly	Greece/Bulgaria	7/24
3	German Interests in Polish Upper Silesia and the Factory at Chorzow	Germany/Poland	5/25
4	Denunciation of the Treaty of November 2, 1865, Between China and Belgium	Belgium/China	11/26
5	The Lotus Case	France/Turkey	1/27
6	Rights of Minorities in Upper Silesia (Minority Schools)	Germany/Poland	1/28
7	Payment of Various Serbian Loans Issued in France	France/Serb-Croat-Slovene State	5/28
8	Payment of Brazilian Federal Loans	France/Brazil	4/28
9	Free Zones of Upper Savoy and the District of Gex	France/Switzerland	3/29
10	Legal Status of Eastern Greenland (includes SE Greenland case)	Denmark/Norway	7/31
11	Delimitation of the Territorial Waters between Castellorizo and Anatolia	Turkey/Italy	5/29
12	Administration of the Prince of Pless	Germany/Poland	5/32
13	Appeal from Judgment of Czechoslovak-Hungarian Mixed Arbitral Tribunal (Peter Pazmany University v. Czechoslovakia	Czechoslovakia/Hungary	5/33
14	Polish Agrarian Reform and the German Minority	Germany/Poland	7/33
15	The Lighthouse Case	France/Greece	5/33
16	Oscar Chinn Case	United Kingdom/Belgium	5/34
17	Pajzs, Csaky and Eterhazy	Hungary/Yugoslavia	12/35
18	Losinger & Co.	Switzerland/Yugoslavia	11/35
19	Diversion of Water from the River Meuse	Netherlands/Belgium	8/36
20	The Borchgrave Case	Belgium/Spain	3/37
21	Phosphates in Morocco	Italy/France	3/36
22	Panevezys-Saldutiskis Railway	Estonia/Lithuania	11/37
23	Electricity Co. of Sofia and Bulgaria	Belgium/Bulgaria	1/38
24	Societe Commerciale De Belgique	Belgium/Greece	5/38

B. Two-Party Cases for the International Court of Justice

	Name of Case	Participants	Date of Introduction
1	Corfu Channel Case	United Kingdom/Albania	5/47
2	Fisheries Case	United Kingdom/Norway	9/49
3	Asylum Case	Columbia/Peru	10/49
4	Rights of Nationals of the U.S. in Morocco	France/United States	10/50
5	Ambatielos	Greece/United Kingdom	4/51
6	Anglo-Iranian Oil Co.	United Kingdom/Iran	5/51
7	The Minquiers and Ecrehos Case	France/United Kingdom	12/50
8	Nottebohm	Liechtenstein/Guatemala	12/51
9	Case of Certain Norwegian Loans	France/Norway	7/55
10	Case Concerning Right of Passage over Indian Territory	Portugal/India	12/55
11	Case Concerning the Application of the Conv. of 1902 Governing the Guardianship of Infants	Netherlands/Sweden	7/57
12	Interhandel Case	Switzerland/U.S.	10/57
13	Case Concerning Sovereignty over Certain Frontier Land	Belgium/Netherlands	11/57
14	Case Concerning Arbitral Award Made by King of Spain on 23 December 1906	Honduras/Nicaragua	7/58
15	Case Concerning Aerial Incident of 27 July 1955	Israel/Bulgaria	10/57
16	Case Concerning the Temple of Preah Vihear	Cambodia/Thailand	9/59
17	Case Concerning the Protection of French Nationals and Protected Persons in Egypt	France/Egypt	10/49
18	"Electricité de Beyrouth" Company Case	France/Lebanon	8/53
19	Treatment in Hungary of Aircraft and Crew of USA	USA/Hungary	2/54
20	Treatment in Hungary of Aircraft and Crew of USA (USSR)	USA/USSR	2/54
21	Aerial Incident of March 10, 1953	USA/Czechoslovakia	3/53
22	Antarctica Case	United Kingdom/Argentina	5/55
23	Antarctica Case	United Kingdom/Chile	5/55
24	Aerial Incident of Oct. 7, 1952	USA/USSR	5/55
25	Aerial Incident of July 27, 1955	USA/Bulgaria	10/57
26	Aerial Incident of July 27, 1955	United Kingdom/Bulgaria	11/57
27	Aerial Incident of Sept. 4, 1954	USA/USSR	7/58
28	Case Concerning the Barcelona Traction, Light and Power Co.	Belgium/Spain	9/58
29	Case Concerning the Compagnie du Port, des Quais et des Entrepots de Beyrouth and the Societé Radio-Orient	France/Lebanon	2/59
30	Aerial Incident of Nov. 7, 1954	USA/USSR	6/59
31	Case Concerning the Northern Cameroons	Cameroon/United Kingdom	5/61

C. Two-Party Cases for the League of Nations

	Name of Case	Participants	Date of Introduction
1	Enzeli	Iran/U.S.S.R.	5/20
2	Aaland Islands	Sweden/Finland	6/20
3	Vilna	Poland/Lithuania	9/20
4	Coto	Panama/Costa Rica	2/21
5	Upper Silesia	Poland/Germany	8/21
6	Eastern Carelia	Finland/U.S.S.R.	0/21
7	Tunis Nationality Decrees	United Kingdom/France	8/22
8	Hungarian Frontier	Hungary/Yugoslavia	5/22
9	Burgenland	Austria/Hungary	8/22
10	Salgo Tarsan	Hungary/Czechoslovakia	11/22
11	Hungarian Optants	Hungary/Romania	3/23
12	Jaworzina	Poland/Czechoslovakia	9/23
13	Corfu	Greece/Italy	9/23
14	Ecumenical Patriarch	Greece/Turkey	2/25
15	Dmir Kapu	Bulgaria/Greece	10/25
16	Albanian Minorities	Albania/Greece	12/25
17	Mosul Territory in Iraq	Turkey/United Kingdom	8/24
18	Cruiser "Salamis"	Greece/Germany	8/25
19	Bahrein Islands	Iran/United Kingdom	1/27
20	Gran Chaco I	Bolivia/Paraguay	12/28
21	Gran Chaco II	Bolivia/Paraguay	9/32
22	Rhodope Forest	Greece/Bulgaria	7/30
23	Manchuria	China/Japan	9/31
24	Bulgarian-Greek Debt	Bulgaria/Greece	0/31
25	Finnish Vessels	Finland/United Kingdom	9/31
26	Iraq-Syrian Frontier	France/United Kingdom	12/31
27	Letica	Peru/Columbia	9/32
28	Anglo-Persian Oil Co.	United Kingdom/Iran	12/32
29	Iraq Frontier	Iran/Iraq	12/34
30	Hungarian Frontier	Hungary/Yugoslavia	5/34
31	Marseilles Crimes	Hungary/Yugoslavia	12/34
32	Syria (Sanjak)	Turkey/France	12/36
33	Ethiopia	Ethiopia/Italy	3/35
34	China	China/Japan	8/37
35	Russo-Finnish War	Finland/U.S.S.R.	12/39

D. Two-Party Cases for the United Nations

	Name of Case	Participants	Date of Introduction
1	Forces in Iran	Iran/U.S.S.R.	1/46
2	Thai Border	Thailand/France	5/46
3	Treatment of Indians	India/S. Africa	6/46
4	Corfu Channel	United Kingdom/Albania	1/47
5	Kashmir I	India/Pakistan	1/48
6	Intervention in China	China/U.S.S.R.	9/49
7	Threats to Yugoslavia	Yugoslavia/U.S.S.R.	11/51
8	Anglo-Iranian Oil Co.	United Kingdom/Iran	9/51
9	Morocco	Egypt/France	10/51
10	Forces in Burma	Burma/Nat. China	3/53
11	West Irian	Indonesia/Netherlands	8/54
12	Syria-Turkish Crisis	Syria/Turkey	10/57
13	Cambodian Border	Cambodia/Thailand	11/58
14	Sudanese Border	Sudan/Egypt	2/58
15	Tunisian Border	Tunisia/France	2/59
16	Laos Intervention	Laos/N. Vietnam	8/59
17	Eichman Kidnapping	Argentina/Israel	6/60
18	South Tyrol	Austria/Italy	6/60
19	Cuban Complaint	Cuba/United States	7/60
20	U-2 Incident	United States/U.S.S.R.	4/60
21	Mauritania	Morocco/France	12/60
22	Cyprus	Greece/Turkey	12/63
23	Goa Invasion	Portugal/India	12/61
24	Venezuelan Boundary	Venezuela/United Kingdom	8/62
25	Senegal Border	Senegal/Portugal	3/63
26	Ethiopian Border	Ethiopia/Somalia	2/64
27	Panama	Panama/United States	1/64
28	Haiti/Dom. Rep.	Haiti/Dominican Republic	5/63
29	Cambodian Border	Cambodia/South Vietnam	4/64
30	Gibraltar	United Kingdom/Spain	8/65
31	Mercenaries in Angola	Rep. of Congo/Portugal	9/66

The Swing Ratio and Game Theory

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It is well known that in electoral systems where a single representative is elected from each district or constituency on the basis of a simple plurality, the proportion of seats in the legislature a party can expect to win is not equal to the proportion of the overall vote it receives. In a two-party system the minority party can expect to be underrepresented, while the party attracting more than 50 per cent of the popular vote usually wins a higher proportion of seats than votes. One indicator of this tendency is the *swing ratio s*, the change in representation per change in popular vote from one election to the next. If r is the proportion of seats a party wins, and p the proportion of the popular vote it receives, $s=\Delta r/\Delta p$, a ratio which has been found to be more or less constant in any one political context as long as p does not deviate too much from the 40–60 per cent range. Predictive models of the relationship between r and p may allow for a degree of statistical fluctuation. Such models predict $\bar{r}$ as a function of p, where $\bar{r}=E(r)$, the expected value of r. Then the quantity of interest is the expected swing ratio, $\bar{s}=E(s)=d\bar{r}/dp$, evaluated at $p=0.5$, i.e., where both parties have 50 per cent of the vote.

In Britain, it has been found[1] that $\bar{s}\approx 3$; in the U.S.,[2] s ranges between 2.5 and 3.3 depending on the mode of calculation and also on the legislative body (House or Senate) being considered. For Quebec, data from 27 elections are best fitted by an s-value of 3.8. One suggestion, that $\bar{s}=3.0$ is characteristic of all political contexts with this type of electoral system, has been formulated in terms of the 'cube law', $\bar{r}=p^3/[p^3+(1-p)^3]$. Unfortunately, no large-scale systematic surveys have been made to test this hypothesis. Nevertheless, it seems clear that s ranges between 2 and 4 and not between 1.0 and 1.2, say, nor between 8 and 16.

The question which has not been satisfactorily answered, and to which this paper is addressed, is this: Why does s tend to take on values only in this particular range, independent of country and time? Kendall and Stuart[3] have shown that any sampling explanation based on the unevenness of distribution of party supporters among constituencies must involve quantitative assumptions which are just as difficult to explain as the swing ratio effect![4] March[5] proposed a reasonable model based on interparty negotiations and intra-constituency pressures; but to quantify this model would require assumptions about the values of a number of parameters. Furthermore the behavior of this model would tend to be rather sensitive to the values chosen for these parameters.

In this paper we define a two-person zero-sum game which is perhaps the simplest possible abstraction of this type of electoral contest. The idea is to try to account for all or part of the swing ratio effect in terms of resource allocation strategies by the two competing parties. There are no parameters to specify, and, as we shall show, under asymptotically optimal strategies, $\bar{s}\approx 2$.

The Game

There are two players, I and II, with resources θ and $1-\theta$, respectively, where $0<1-\theta<\theta<1$. There are n boxes and each player must allocate, secretly, his resources among the boxes. After this allocation is completed, the amount I put in each box is compared with the amount II put in, and whoever put more, wins the box. The payoff is the proportion of boxes won by I. Player I tries to maximize the minimum payoff he can expect for any strategy II may play; while II tries to minimize the maximum payoff expected for any strategy available to I.

The particular case of this game where $n=3$ was discussed by Owen,[6] but only for θ fixed at 0.5. The general case has not been discussed previously.

This game does not have a saddle point in pure strategies, except for the trivial case $\theta/n>1-\theta$. We prove this in the next section and show that even as $n \to \infty$, i.e., for large numbers of constituencies, the game has no solution or approximate solution in pure strategies. Extending the game to allow mixed strategies, we show how to construct strategies depending on θ and n in a way which is asymptotically optimal as $n \to \infty$.

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[1] M. G. Kendall and A. Stuart, "The Law of the Cubic Proportion in Election Results," *British Journal of Sociology*, 1 (September, 1950) 183–197.

[2] J. G. March, "Party Legislative Representation as a Function of Election Results," *Public Opinion Quarterly*, 21 (Winter, 1957–58), 521–542.

[3] Kendall and Stuart, "The Law of the Cubic Proportion . . ."

[4] A recent study which assumes the cube law is Henri Theil, "The Cube Law Revisited," *Journal of the American Statistical Association*, 65 (September, 1970) 1213–1219.

[5] March, "Party Legislative Representation . . ."

[6] G. Owen, *Game Theory* (Philadelphia: W. B. Saunders, 1968), pp. 88–93.

Following this analysis we devote a section to its interpretation in electoral terms.

Mathematical Analysis[7]

THEOREM 1. For any pure strategy available to II, player I has an opposing pure strategy producing a payoff of 1. For any pure strategy available to I, II has an opposing one producing a payoff less than or equal to $1-(1-\theta)/\theta+1/n$.

Proof. Let $y=(y_1, \cdots, y_n)$ be any pure strategy available to II; i.e., y_1 is allocated to the first box, y_2 to the second, etc. For $i=1, \cdots, n$, set $x_i=y_i+(2\theta-1)/n$. Then $x=(x_1, \cdots, x_n)$ is a strategy for I because

$$\sum_{i=1}^{n} x_i = \theta,$$

and the payoff for x against y is 1 since $x_i > y_i$ for $i=1, \cdots, n$.

Now let x be any pure strategy available to I. Without loss of generality we can assume

$$x_1 \leq x_2 \leq \cdots \leq x_n. \tag{1}$$

Let m be such that

$$\sum_{i=1}^{m} x_i < 1-\theta, \qquad \sum_{i=1}^{m+1} x_i \geq 1-\theta. \tag{2}$$

Clearly $m \geq 1$ unless $\theta/n \geq 1-\theta$, in which case the theorem is trivially true. Otherwise set

$$y_i = \begin{cases} x_i + \left(1-\theta-\sum_{j=1}^{m} x_j\right) \Big/ m, & \text{for } i = 1, \cdots, m \\ 0, & \text{for } i = m+1, \cdots, n. \end{cases}$$

The payoff for x against $y=(y_1, \cdots, y_n)$ is $1-m/n$. An upper bound for this quantity can be found as follows: From (1) and the second inequality of (2), we have

$$x_{m+1} \geq \frac{1-\theta}{m+1}.$$

By (1), this inequality must also be satisfied by each of $x_{m+2}, \cdots, x_n$. Thus

$$[n-(m+1)]\,\frac{1-\theta}{m+1} \leq \sum_{i=m+2}^{n} x_i. \tag{3}$$

But from (2),

$$\sum_{i=m+2}^{n} x_i \leq \theta-(1-\theta). \tag{4}$$

Inequalities (3) and (4) together yield

$$n\,\frac{1-\theta}{m+1}-(1-\theta) \leq \theta-(1-\theta),$$

$$1-m/n \leq 1-(1-\theta)/\theta+1/n.$$

This theorem shows that the game with pure strategies has no saddle point (except for trivial cases) and that when this game is played with many boxes, it is as far away from having a solution as when it is played with a few.

In contrast to this, we show in Theorem 2 how to construct a pair of *mixed* strategies which come within $3/2n$ of constituting a solution. For large n, such as encountered in electoral systems (say 100–1000), these strategies specify, for all practical purposes, a value for the game. Before proving this theorem we require some definitions.

For pure strategies x versus y the payoff to I will be denoted

$$A(x, y) = (1/n)\sum_{i=1}^{n} \chi(x_i, y_i),$$

where

$$\chi(a, b) = \begin{cases} 1, & \text{if } a > b \\ \tfrac{1}{2}, & \text{if } a = b \\ 0, & \text{if } a < b. \end{cases} \tag{5}$$

A mixed strategy for a player may be considered as a probability distribution over all the strategies available to him. Thus we work with the set of cumulative distribution functions on the n-dimensional interval $U=[0, 1]^n$ which concentrate all their mass on the hyperplane defined by

$$\sum_{i=1}^{n} x_i = \theta$$

in the case of player I, or

$$\sum_{i=1}^{n} y_i = 1-\theta$$

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[7] We would like to thank Professors Mark Kac, Jean-Jacques Moreau, and Frank Stenger for advice, and Professor Guillermo Owen for information about this type of game. As far as possible we shall keep to the terminology and notation of Owen, *Game Theory*.

in the case of II. Since we are using mixed strategies, we are interested not just in the payoff, but in the *expected* payoff

$$E(F, G) = \int_U \int_U A(x, y)\,dF(x)\,dG(y) \qquad (6)$$

where I plays mixed strategy F and II plays G. If G assigns probability 1 to the pure strategy y

$$E(F, G) = E(F, y) = \int_U A(x, y)\,dF(x)$$

and an analogous remark may be made about $E(x, G)$. The values of the game[8] for I and II are v_I and v_{II} respectively where

$$\sup_F \inf_y E(F, y) = v_I \le v_{II} = \inf_G \sup_x E(x, G).$$

THEOREM 2.

$$v_I + 1/2n \geq (3\theta - 1)/2\theta \geq v_{II} - 1/n.$$

Proof. Let F be the strategy for I which assigns probability $1/n!$ to each permutation of the vector $(u, 3u, \ldots, [2n-1]u)$ where $u=\theta/n^2$. Let H be any mixed strategy for II. From (5) and (6)

$$\begin{aligned} &E(F, H)\\ &= (1/n)\int_U\int_U \sum_{i=1}^{n} \chi(x_i, y_i)\,dF(x)\,dH(y) \qquad (7)\\ &= (1/n)\int_U \sum_{i=1}^{n}\sum_{j=1}^{n} (n-1)!\\ &\qquad\cdot\chi([2j-1]u, y_i)/n!\,dH(y) \end{aligned}$$

since F assigns probability $1/n!$ to $n!$ different vectors, $(n-1)!$ of which have $[2j-1]u$ as the i-th coordinate, for each $j=1, \ldots, n$. Then

$$\begin{aligned} &E(F, H) \qquad (8)\\ &= (1/n^2)\int_U \sum_{i=1}^{n}\sum_{j=1}^{n} \chi([2j-1]u, y_i)\,dH(y)\\ &\geq (1/n^2)\int_U \sum_{i=1}^{n} (n-(y_i+u)/2u)\,dH(y), \qquad (9) \end{aligned}$$

since $\chi([2j-1]u, y_i)=1$ except when $y_i \geq [2j-1]u$; i.e., except for $j=1, \ldots, r$ where r is the largest integer in $(y_i+u)/2u$. For H to be a strategy for II, it must concentrate all its mass on the hyperplane

$$\sum_{i=1}^{n} y_i = 1 - \theta,$$

where the integrand in (9) becomes $n^2-(1-\theta)/2u-n/2$. Therefore

$$\begin{aligned} &E(F, H)\\ &\geq (1-(1-\theta)/2n^2u - 1/2n)\int_U dH(y) \qquad (10)\\ &= (3\theta-1)/2\theta - 1/2n, \end{aligned}$$

which is a lower bound for v_I, since H was chosen arbitrarily.

Similarly, let G assign probability $(n-m)!/n!$ to each distinct permutation of $(w, 3w, \ldots, [2m-1]w, 0, \ldots, 0)$ where $w=(1-\theta)/m^2$ and m is the largest integer in $n(1-\theta)/\theta$. Let H be any mixed strategy for I. As in (7)–(10),

$$\begin{aligned} &E(H, G)\\ &= (1/n^2)\int_U \sum_{i=1}^{n}\left[\sum_{j=1}^{m} \chi(x_i, [2j-1]w) + \sum_{j=m+1}^{n} \chi(x_i, 0)\right] dH(x)\\ &\leq (1/n^2)\int_U \sum_{i=1}^{n}[(x_i+w)/2w + n - m]\,dH(x)\\ &= 1 + m[2n - m\theta/(1-\theta)]/2n^2 + 1/2n. \end{aligned}$$

Using the fact that $n(1-\theta)/\theta-1 < m \leq n(1-\theta)/\theta$, it follows that

$$E(H, G) \leq (3\theta-1)/2\theta + 1/n,$$

and this completes the proof.

Electoral Interpretation

Theorem 2 shows that in a game with many boxes, if I and II are playing nearly optimal strategies (or optimal strategies if these exist), the average payoff will be approximately $(3\theta-1)/2\theta$.

How can this game be related to electoral systems? The n boxes, of course, represent n constituencies, and the payoff represents the proportion of these constituencies won by the majority party. The simplest way to interpret the resource variable θ is to equate it to p, the proportion of the total popular vote won by the majority party. This leads immediately to the prediction $\bar{r}=(3p-1)/2p$, and $\bar{s}$, evaluated at $p=0.5$, is 2. Thus, if the parties in

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[8] Optimal strategies exist if

$$\max_F \min_y E(F,y) = v = \min_G \max_x E(x,G),$$

in which case v is called the value of the game.

a two-party system could freely allocate their total support among the constituencies, we should expect a swing ratio equal to 2.

This model is, of course, excessively simple. It would be preferable to relate the variable θ to the sum total of electoral resources: financial, organizational, press goodwill, etc. Some of these resources can be freely allocated; others, however, are bound to certain constituencies. If we were somehow able to identify θ with the freely allocatable resources, there would still remain the problem of predicting the vote in each constituency. Part of the resources bound to a particular constituency is the proportion of hard-core voters. A realistic model would have to combine[9] the hard-core vote, which is relatively independent of free resources allocated, with the vote of the previously uncommitted constituents, which is more proportional to free resources allocated to the constituency.

It is easy to see how such a model would have a higher swing ratio than the pure game-theory model. Assuming the hard-core vote is $(1-z)/2$ for both parties in most constituencies, the game would be played to capture a majority of the uncommitted vote in as many constituencies as possible. Then $\bar{r}=(3\theta-1)/2\theta$ as before, but instead of $p=\theta$, we have $p=\theta z+(1-z)/2$, and hence $\bar{s}=2/z$ when $\theta=p=1/2$. So if one third of the voters, say, are hard-core, $\bar{s}=3$.

Parameters other than z, perhaps related to differences between constituencies or to the turnout proportion, could also be added to the game. Some of these would tend, like z, to amplify the expected swing ratio, and others would tend to attenuate it. The important point about the game model is that it gives a reasonable basic value of $\bar{s}$ without the introduction of *any* parameters. It is a very simple model. Nevertheless, we feel that it summarizes the strategic problem faced by parties in this type of electoral system and that the asymptotic solution captures the most important component of the swing ratio effect.

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[9] Thiel, "The Cube Law Revisited," shows one way of doing this.

The Strategy of Ambiguity: Uncertainty and Electoral Competition*

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Nicholas Biddle, the manager of William Henry Harrison's campaign for the presidency, is reported to have given his candidate the following advice:

> Let him say not one single word about his principles, or his creed—let him say nothing—promise nothing. Let no Committee, no convention—no town meeting ever extract from him a single word, about what he thinks now, or what he will do hereafter. Let the use of pen and ink be wholly forbidden as if he were a mad poet in Bedlam.[1]

More recently, presidential candidates have followed Biddle's seemingly ageless advice with ambiguous pronouncements on the salient political issues of the time.[2] In the 1968 presidential campaign, both Nixon's "I have a plan" statements on the Vietnam issue and Humphrey's "law and order with justice" slogan on "the social issue" suggest that the equivocal pronouncement during the course of a campaign is a common and recurrent theme in American electoral politics. Sartori places this phenomenon in a still broader context:

> In his pungent essay, *Politics and the English Language*, George Orwell pointed out that 'the great enemy of clear language is insincerity' and that there is therefore a 'special connection between politics and the debasement of language [Orwell, *Selected Essays*, (London, 1957), pp. 152, 154, 157].' Ordinary political language is insincere language, for it is less an instrument for expressing thought than a means 'for concealing or preventing thought.' Actually, except in the case of some intellectuals, political discourse is more often than not either a device for achieving and maintaining power, or a device for stimulating action . . . . I do not wish to sound cynical. I am only trying to point out the special difficulties that a student of politics is bound to encounter. *For he has to work in a field where it may be considered useful to becloud problems, and definitely not useful to clarify them. Indeed, an important part of politics may be described as the art of confusing political issues (and this is not necessarily the wicked part).*[3]

That candidates are seen as vague and ambiguous in their campaigning and that the observed ambiguity typically involves precisely those issues on which the election hinges may discourage the traditional democrat. The ballot, a fundamental feature of democratic government, presumably provides an opportunity for the citizen to register his preferences on the important issues facing the collectivity. Citizens, however, vote for candidates, not policy alternatives. Ambiguous campaigning, reasons the traditional democrat, therefore compromises the "ballot box principle" by preventing citizens from identifying candidates with policy positions.

The aim of this paper is to explicate the rôle of ambiguity in electoral competition. I examine the effects of uncertainty on the decisions of voters and the strategy choices of candidates. The underlying theme here is the basic incompatibility of the hopes of the traditional democrat on the one hand and the desires of the politician on the other. It is argued that the rules of the democratic game often conflict with underlying normative principles—that incentives to equivocate are perceived by candidates for office, much to the consternation of the "responsible democrat."

A second aim of this paper is the introduction of mathematical decision theory and its integration with formal models of political phenomena. Choice, it is maintained, is the basic social act that transforms the essentially private thoughts and values of individuals into "public activity." And since "public activities" comprise the set of observables that concerns the behavioral scientist, a theory of choice—decision theory— is a most appropriate tool of analysis and theory building. In particular, a theory of decision making in contingencies of risk and uncertainty permits us to consider rational actors who are constrained

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* Several closely related papers including the one presented here have benefitted from close readings and careful criticism by a number of people. For their efforts I acknowledge and thank Richard Niemi, Alvin Rabushka, William Riker, John Sprague, and Herbert Weisberg, as well as the anonymous referees of this paper. A Washington University Faculty Summer Research Grant provided time to prepare and revise various drafts of this paper.

[1] Nicholas Biddle, *Correspondence*, R. C. McGrane, ed. (Boston:Houghton, Mifflin, 1919), p. 256. Cited in Arthur M. Schlesinger, Jr., *The Age of Jackson* (Boston: Little, Brown, 1945), p. 211. I thank Professor Robert Lineberry for bringing this citation to my attention.

[2] For example, William Leuchtenberg reports the following remarks by a Harding advisor during the 1920 presidential campaign: "From his deathbed, Boies Penrose the Pennsylvania machine politician advised: 'Keep Warren at home. Don't let him make speeches. If he goes out on tour somebody's sure to ask him questions, and Warren's just the sort of damned fool that will try to answer them.' " William E. Leuchtenberg, *The Perils of Prosperity, 1914–1932* (Chicago: University of Chicago Press, 1958), p. 88. I thank Risé S. Shepsle for this citation.

[3] Giovanni Sartori, *Democratic Theory* (Detroit: Wayne State University Press 1962), p. 207 (emphasis added).

in their choice behavior by environmental limitations on information as well as by their preference structures.

Theoretical Precursors

A concern with ambiguity in electoral competition is not novel. Various students of politics have acknowledged the manifestations and consequences of ambiguity, minimally with guarded pessimism, and more often with outright anxiety. "Why," ask Pendleton Herring, to cite an example,

> cannot there be more direction and less confusion in our political life? Surveying the history of our parties, we find discord among their members and frequent evasion in their platforms. Our parties have generally side-stepped the issues that have divided the nation most deeply.[4]

Or, to cite another example, V. O. Key, Jr., after observing that ". . . politicians prefer to be sure that if they act they will not find themselves far out on a limb,"[5] suggests that owing to the unpredictable reactions of the electorate to volatile, salient issues, politicians become "addicted to equivocation and ambiguity."[6]

For yet another example, one need only peruse the pages of Downs's *An Economic Theory of Democracy*. There he argues—and this argument is examined carefully below—that on the "critical issues" parties perceive incentives to equivocate, to "becloud their policies in a fog of ambiguity."[7] The disturbing consequence of this kind of behavior, Downs claims, is that parties are led to appeal to the irrational in the electorate. The "fundamental tension" in his model—the conclusion that "rational behavior by political parties tends to discourage rational behavior by voters"[8] —produces difficulties for a governmental arrangement in which the governors are to be responsive to the wishes and preferences of the governed.

As these examples suggest, the strategy of ambiguity in electoral competition has been a source of empirical interest and normative concern for a wide range of scholars.[9] To those we have cited above, one might add a host of economists and management scientists whose interests include the behavior of firms, consumers, and investment markets in an uncertain world.[10] The ambiguity of signals, whether its source is the platform and pronouncements of a political party, the pricing and production decisions of a firm, or the ever variable movements of the Dow-Jones average, pervades many aspects of social life.

Scholarly attention to the nature and implications of ambiguity and, more generally, of uncertainty is widespread.[11] But with the notable exception of Downs, little serious, systematic attention has been devoted to its impact and consequences in the political realm.[12] This paper examines one kind of uncertainty in the electoral context. The theory proposed below is formal, in effect a slight mathematical generalization of Downs's model of party competition. Its central feature is the exploitation of formal theories of decision making under risk.

The Theory of Ambiguity: A Generalization of Downs

In the Downsian model, it will be recalled, there are two kinds of actors—citizens and candidates for office—each motivated by different sets of incentives. The Downsian citizen has preferences about issues of public policy and uses his resources (or chooses not to) to affect the direction of government policy. Citizens are assumed to be rational in the sense that (1) they are capable of making choices whenever alternatives are placed before them (connectivity),[13] and (2) the choices they make possess a certain modicum of consistency (transitivity).[14] In effect, then, each citizen in Downs's model is represented by a *preference ordering* defined over public policy issues.

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[4] Pendleton Herring, *The Politics of Democracy: American Parties in Action*, 2nd ed. (New York: Norton, 1965), p. 188.

[5] V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1961), p. 424.

[6] V. O. Key, Jr., *Politics, Parties and Pressure Groups*, 4th ed. (New York: Crowell, 1958), p. 241.

[7] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 136.

[8] Downs, p. 136.

[9] For an effort to explicate the role of ambiguity in the context of administrative decision making, a paper recently brought to my attention is Julian Wolpert's "Consequences of Inadvertent and Purposeful Ambiguity in Locational Strategies," presented at the 65th Annual Meeting of the American Political Science Association, New York 1969.

[10] Three monographs provide a sampling of this literature. See Karl Henrik Borch, *The Economics of Uncertainty* (Princeton: Princeton University Press, 1968); Howard Raiffa, *Decision Analysis* (Reading, Mass.: Addison-Wesley, 1968); and Kenneth J. Arrow, *Essays on the Theory of Risk-Bearing* (Chicago: Markham, 1971).

[11] The importance of these inquiries is suggested in a passage from a paper by Arrow:

> There is no need to enlarge upon the importance of a realistic theory explaining how individuals choose among alternate courses of action when the consequences of their actions are incompletely known to them. It is no exaggeration to say that every choice made by human beings would meet this description if attention were paid to the ultimate implications. Risk and the human reactions to it have been called upon to explain everything from the purchase of chances in a "numbers" game to the capitalist structure of our economy; according to Professor Frank Knight, even human consciousness itself would disappear in the absence of uncertainty.

See Kenneth J. Arrow, "Alternative Approaches to the Theory of Choice in Risk-Taking Situations," *Econometrica*, 19 (October 1951), 404. Also see Frank H. Knight, *Risk, Uncertainty, and Profit* (New York: Houghton-Mifflin, 1921).

[12] This statement needs some qualification if the specialized literature in the field of constitutional choice—essentially the social choice of a social decision process under conditions of uncertainty—is included. See, for example, James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962); and Douglas W. Rae, "Decision Rules and Individual Values in Constitutional Choice," *American Political Science Review*, 63 (March 1969), 40–56.

The typical candidate of the Downsian model polity is an office-seeker. His behavior is determined solely by the effect it is likely to have on his quest for office. "Politicians in our model are motivated by the desire for power, prestige, and income . . . their primary objective is to be elected."[15] A candidate's own policy preferences are of marginal importance since "politicians . . . never seek office as a means of carrying out particular policies; their only goal is to reap the rewards of holding office *per se*."[16]

The mechanism that captures the dynamic of electoral competition is the well-known spatial model. Since electoral outcomes depend upon the aggregation of voter choices and since individual voter choices depend upon individual preferences, candidates for office choose policy positions and utter policy-related statements with an eye to voter preferences and the likely aggregate of voter choices. Stokes characterizes Downs's one-dimensional version of the spatial model well:

> The root idea of Downs's model is that the alternatives of government action on which political controversy is focused can be located in a one-dimensional space, along a left-right scale. At least for illustration, Downs interprets this dimension as the degree of government intervention in the economy . . . . Each voter can be located on the scale according to how much government control he wants and each party according to how much government control it advocates.[17]

Each voter is characterized by a point on the one-dimensional space (his most-preferred point) and by a preference metric which relates "distance" from his most-preferred point to preference. Each candidate for office is simply a single point in the space. With appropriate assumptions (see below) about the preference metric the voter casts his vote (if he votes at all) for the candidate who locates himself "closest" to the voter's most-preferred point.

In order to generalize Downs's model for the purposes of examining the effects of uncertainty, it is useful to begin with an explicit recognition of the assumptions required. Typically, spatial models begin with assumptions about the space of available policy alternatives, the nature of preferences defined on the space, and motives for choice. For the purposes of this paper the following assumptions are made:

(boundedness and unidimensionality) A.1: The space of alternatives is a bounded continuum, [A, B].

(single-peakedness) A.2: Each v_i in the set of citizens $V = \{v_1, v_2, \cdots, v_n\}$ possesses a continuous, differentiable (at all but at most a finite number of points), single-peaked, von Neumann-Morgenstern preference function $u_i(x)$ ($x \in [A, B]$), consistent with A.3.

(quasisymmetry) A.3: The metric "Euclidean distance from most preferred point" induces a preference relation such that

$$\rho(y, x_i) \underset{<}{\overset{>}{-}} \rho(z, x_i) \Rightarrow u_i(y) \underset{>}{\overset{<}{-}} u_i(z),$$

where ρ is the Euclidean distance metric and x_i is the most-preferred point of v_i.

(utility maximization) A.4: Citizens are nonstrategic (expected) utility maximizers.

(universal participation) A.5: All citizens vote.

(entrepreneurial incentives) A.6: Political entrepreneurs employ lexicographic criteria to guide their behavior: they are primarily concerned with winning office; a secondary concern is maximizing votes.

The first assumption is, in a sense, restrictive but does not detract from the major points to be made below. The restrictive feature of this assumption is, of course, unidimensionality. Elections typically involve a myriad of issues for which A.1 and A.2 are not compatible. For the purposes of making several points about the utility of ambiguous strategies, however, it is convenient to assume unidimensionality.[18] The boundedness assumption adds no difficulties and is theoretically quite general.[19] In particular, it permits without loss of generality the consideration of that subinterval of the policy continuum found between the "extreme voters" most preferred points in either direction.

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[13] If alternatives A and B are on the agenda, then each citizen either prefers A to B, B to A, or is indifferent between them.

[14] Whereas connectivity is a dyadic property, transitivity imposes constraints on triads of alternatives. Thus, for the dyad (A, B), if a citizen prefers A to B or is indifferent; if, for the dyad (B, C), he prefers B to C or is indifferent; then it cannot be the case that he prefers C to A when the dyad (A, C) is offered. That is, for (A, B, C), $A \geq B$ and $B \geq C \rightarrow \sim (C > A)$, where $\geq$ is the symbol for preference or indifference.

[15] Downs, *Economic Theory of Democracy*, p. 30.

[16] Downs, p. 28.

[17] Donald E. Stokes, "Spatial Models of Party Competition," *American Political Science Review*, 57 (June 1963), 368–377 (quoted at p. 368).

[18] However, one is not required to suppose this dimension is ideological in nature, as Downs does. The prospects for relaxing this assumption are ex-

Assumption A.2—single-peakedness—requires that all voters possess a "bliss point" (most preferred point), with preference or utility decreasing for points increasingly distant from that point.[20] Assumption A.3 specifies the relationship between distance and preference, namely that Euclidean distance from most preferred point and preference are negatively correlated. In combination, then, A.2 and A.3 require a set of voters with single-peaked utility functions, for some particular ordering of the alternatives, such that the Euclidean distance metric provides a measure of preference. An electorate conforming to A.2 and A.3 is called an *electoral profile*.[21]

Assumption A.4 requires little comment except the emphasis of the word "nonstrategic." It is assumed that the election contest involves individual citizens comparing the alternatives available and deciding on how to use their vote by appealing to their individual preferences. Neither cooperative-collusive behavior nor "sophisticated" voting is considered. That is, the Downsian model, and ours as well, is equivalent to a two-person zero-sum game played by the competing candidates for office. The voters are *not* players in the game; rather, they serve a more mechanistic role, providing signals and structuring the payoff function for the "actual" participants—the candidates for office.[22]

That nonvoting has important implications for entrepreneurial behavior, in both the strategic sense (e.g., policy pronouncements) and the tactical sense (e.g., canvassing and other precinct-level activities) has been suggested by Garvey, Hinich, Kramer, Ordeshook, and others.[23] As their research has shown, however, abstention is an incredibly complex phenomenon to model. We avoid these complexities by postulating A.5: universal participation. Our results must be interpreted with this restriction in mind.

Finally, we assume that political entrepreneurs are office seekers and choose electoral strategies with this goal in mind. Among those behaviors which are perceived as equivalent in terms of office seeking, a secondary criterion, vote maximization, is employed. Plurality maximization might have been employed as the secondary criterion instead since, by A.5, vote maximization and plurality maximization are equivalent. In any event, assumption A.6 renders votes the only political currency of interest to the candidates for office.[24]

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tremely good, as has been indicated by the multidimensional generalizations of Downs in the research of scholars at Carnegie-Mellon University. For a summary of this work, see Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June 1970), 426–448; and Melvin Hinich, John Ledyard, and Peter Ordeshook, "A Theory of Electoral Equilibrium: A Spatial Analysis Based on the Theory of Games" (Carnegie-Mellon University, 1971, unpublished manuscript).

[19] In fact, it may be shown that the competitive situation in an unbounded space may be transformed to a bounded space with no effect on strategic considerations. See Kenneth A. Shepsle, *Essays on Risky Choice in Electoral Competition* (unpublished doctoral dissertation, University of Rochester, 1970), Chap. 4. Also see Robert G. Samberg, "Collective Decision-Making in a Bounded Prospect Space," a paper delivered at the Sixty-sixth Annual Meeting of the American Political Science Association, Los Angeles, 1970.

[20] For early discussion of the single-peakedness concept, see Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958). For a generalization of single-peakedness, see Amartya Sen, "A Possibility Theorem on Majority Decisions," *Econometrica*, 34 (April 1966), 491–499.

[21] The term is adapted from Benjamin Ward, "Majority Voting and Alternative Forms of Public Enterprise," in Julius Margolis, ed., *The Public Economy of Urban Communities* (Baltimore: The Johns Hopkins University Press, 1965), pp. 112–126.

[22] For an examination of the dynamics of building a majority coalition, and a less "mechanistic" treatment of the voter, see Steven Brams, "Positive Coalition Theory: The Relationship Between Postulated Goals and Derived Behavior," in Cornelius P. Cotter et al., eds., *Political Science Annual: Conflict, Competition, and Coalitions*, IV (Indianapolis: Bobbs-Merrill, 1972), forthcoming. "Sophisticated voting" as treated in Robin Farquharson, *Theory of Voting* (New Haven: Yale University Press, 1969) is also not considered here. Voters consult only their own preference schedules in making their voting choice. They do not engage in speculation or calculation about the behavior of other voters. In the context of two-candidate elections (which is all we consider in this paper), with single-peaked preference functions and universal participation (A.5), "sophisticated voting", is not crucial. For electoral contingencies in which it is important, see, in addition to Farquharson, Gerald H. Kramer, "Sophisticated Voting Over Multidimensional Choice Spaces," a paper delivered at the Meetings of the Public Choice Society, Blacksburg, Virginia, 1971.

[23] Gerald Garvey, "The Theory of Party Equilibrium," *American Political Science Review*, 60 (March 1966), 29–38; Melvin Hinich and Peter Ordeshook, "Abstentions and Equilibrium in the Electoral Process," *Public Choice*, 7 (Fall 1969), 81–106; Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in Joseph L. Bernd, ed., *Mathematical Applications in Political Science*, II (Dallas: Southern Methodist University Press, 1966), pp. 137–160.

[24] We do not need to be more explicit in stating this assumption since, in the remainder of this paper, we shall be concerned only with *winning* strategies—those strategies that are "dominant" in a sense to be specified below.

Assumptions A.1 through A.6 define a variant of Downs's model of party competition and form the basis for generalization below. Other assumptions that may arise are made explicit wherever practical.

Although it has been subject to a good deal of criticism, Downs's model has survived more than a decade as a basis for the formal examination of electoral processes and governmental decision making. Moreover, it has generated a great deal of research, some of which has been cited, that attempts to remove weaknesses and relax excessively restrictive formulations. Our essay follows this pattern. We examine a structural feature of the environment—the *risk environment*—and its implications for voter and candidate decision making.

Given A.1–A.6, suppose that candidates for office are represented not by points in [A, B] but rather by *probability distributions* over points in [A, B]. That is, candidates are perceived by voters as *lottery tickets* over policy alternatives, and voters (like gamblers) must choose between uncertain prospects.[25] Candidates, on the other hand, must choose their electoral strategies from the set of all probability distributions over [A, B] i.e., from the set P[A, B].

It is quite clear that our conceptualization of candidates as probability distributions over [A, B] effects a simple mathematical generalization of Downs's original treatment since any point $x \epsilon [A, B]$ is a degenerate probability distribution. That is, the new strategy set available to candidates includes the original one proposed by Downs: $[A, B] \subset P[A, B]$. However, not only is there mathematical generality afforded by this conceptualization; additional substantive scope is purchased as well.

First, we can accept with Downs the assumption that politicians do not lie—that false information does not enter the communications system—while still acknowledging the politician's advantage in speaking "half truths" and in varying his appeals with variations in audience and political climate. Our new arrangement permits a device for capturing these incentives to equivocate and provides a method (see below) for specifying the electoral contingencies in which they are likely to be found.

A second justification for this peculiar treatment of electoral competition follows from the non-teamlike behavior of political parties. Downs chose to treat the competition for office as a contest between *teams* of activists. The internal workings of each team are assumed to involve complete cooperation among individuals with identical preferences so that the team may be treated as a unified entity. The simplification effected, however, ignores the "deviant" behavior of the candidate whose official career depends on an electorate different from those of his team colleagues. This observation "underscores the limits to the empirical usefulness of models that assume that party leaders always act in common . . . . An awareness that party leaders (i.e., politicians) act for private motives ought to prompt an extension of models of rational behavior. . . ."[26] The lottery concept permits us to retain (if we wish) the team notion by tempering its unique, unambiguous identity.[27]

Third, the introduction of uncertainty through our probabilistic representation of candidates permits the formal inclusion of limitations in voter information. Possessing neither complete nor perfect information, the voter is simply uncertain of the policies a candidate represents. At best the voter possesses enough information to develop some educated expectations. This limitation is captured by a probabilistic description of electoral possibilities.[28]

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[25] Decision theories ordinarily distinguish among contingencies of certainty, risk, and uncertainty. In the case of *certainty*, each action or choice is known to lead to a specific (set of) outcome(s). In contingencies of *risk*, each action is probabilistically related to outcomes in the sense that action a_i results in outcome o_j with probability p_{ij}. Thus, for the set of outcomes $O = \{o_1, o_2, \ldots, o_n\}$, the i^{th} action or choice is a *lottery ticket:* $a_i = (p_{i1}\, o_1, p_{i2}\, o_2, \ldots, p_{in}\, o_n)$, where the usual probability constraints on the p_i's apply. In the case of uncertainty, each action is probabilistically related to outcomes, but the nature of the relationship depends on an *unknown* parameter Θ, labeled "the state of nature." Thus, $a_i = p_{i1}{}^{(\theta)}\, o_1, p_{i2}{}^{(\theta)}\, o_2, \ldots, p_{in}{}^{(\theta)}\, o_n)$. We have argued elsewhere that the three-part distinction above is, for our purposes, somewhat artificial, and that we may conceive of all contingencies as falling under the rubric of risk—a known probability distribution over outcomes. We shall, however, often use the common parlance, referring (somewhat confusingly, we admit) to uncertainty. *Let us reiterate that we shall always mean risk.* See Kenneth A. Shepsle, "Parties, Voters, and the Risk Environment: A Mathematical Treatment of Electoral Competition Under Uncertainty," in *Probability Models of Collective Decision-Making*, Richard G. Niemi and Herbert F. Weisberg, eds. (Columbus: Charles E. Merrill), forthcoming.

[26] Robert Schoenberger, "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-Making in the 1964 Congressional Elections," *American Political Science Review*, 63 (June 1969), 515–520 (quotation at pp. 519–520).

[27] In fact, throughout this essay we refer to competition among candidates rather than teams of activists.

[28] It should be noted here that, by virtue of A.4 and A.5, voters do not concern themselves, in a strategic fashion, with the effect of their vote on the aggregate outcome. Rather they treat the voting decision as one of individual decision making, maximizing *expected* utility. Thus, the voter's information about the intentions of other voters (an additional source of uncertainty) is not germane in this model. For an

Finally, the introduction of uncertainty via the lottery representation permits an explicit demarcation of the "scope of conflict." Far too frequently, spatial models of electoral competition permit *complete spatial mobility*.[29] When this is not the case, the restrictions on mobility tend to be somewhat arbitrary and unsystematic.[30] In this paper candidates and their strategic options are distinguished according to their electoral role. In particular, we distinguish between incumbent and challenging candidates. The incumbent (or representative of the incumbent party) has been the prime political mover during his period of incumbency. Not only is his visibility enhanced by the media, which transmit his messages (intended or otherwise) to the voter; in addition, he is in a position to communicate directly with the voters through his control of government activities which alter their utility streams.[31] The challenger, on the other hand, is an advocate. Neither his image nor his intentions, we suppose, are well-known to the electorate on election day. We formalize these role distinctions with the following strategic *constraints* on the actors:

(strategic constraints) C.1: The incumbent is restricted in his choice of electoral strategy to *degenerate* probability distributions. We write his available strategies as

$$S_{\mathrm{I}} = \{x/x \epsilon [A, B]\}.$$

C.2: The challenger is restricted in his choice of electoral strategy to *nondegenerate* probability distributions. We write his available strategies as

$$S_{\mathrm{II}} = \{p(x)/x \epsilon [A, B] \text{ and } p(x) \neq 1 \text{ for any } x\}.$$

In addition, we add a further assumption about voters:

(information consensus) A.7: All v_i

(i) perceive the incumbent's position as a certainty;

(ii) perceive the challenger as inherently risky; and

(iii) agree about their perceptions, although they will, in general, differ on preferences.

With A.7, then, Stokes's criticism of a common perceptual space is acknowledged and disposed of by assumption.[32,33]

Before tracing some of the logical implications of this model, the *risk environment* is formally specified. To do so, it is useful to examine one behavioral implication.

Voter Choice and the Risk Environment

Our argument to this point is that the act of voting, like that of gambling or purchasing insurance, is one involving "risky" alternatives. Or, to put the thesis somewhat differently, at least one (and possibly both for constraints other than C.1) of the alternatives (candidates) in the voter's choice set is inherently ambiguous. From the point of view of the candidate, the strategic problem at hand is that of selecting a (possibly risky) strategy from his set of permissible strategies, S_i, $i = \mathrm{I, II}$. In effect, then, we have replaced the Downsian candidate of the certain world—a point on the policy continuum—with the risky candidate. Analytically, the risky candidate is a probability distribution or *lottery ticket* defined on [A, B].

Given this representation the *risk environment* is identified by two characteristics. First, the nature of risky competition depends on the *shape of voter utility functions*. A voter's evaluation of a risky alternative—via the expected utility hypothesis (A.4)—depends on the functional form of his utility schedule.[34] To see this, consider three voters (Figure 1) each of whom prefers x to y and y to z (and x to z by transitivity). Further suppose y is the midpoint of the interval $[x, z]$. Note that although voters a, b and c have identical preference *orderings* over the alternatives, the *shapes* of their utility functions differ.

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initial examination of the voting decision in contingencies of strategic interaction among voters, see William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March 1968), 25–42.

[29] This is analogous to the assumption of free entry and exit in economic models. For an interesting point of view on this assumption, see Albert O. Hirschman, *Exit, Voice and Loyalty: Responses to Decline in Firms, Organizations, and States* (Cambridge: Harvard University Press, 1970).

[30] More recently efforts have been made to deal with this question in a more systematic fashion. See Peter Aranson and Peter Ordeshook, "Spatial Strategies for Sequential Elections," in Niemi and Weisberg, *Probability Models;* James S. Coleman, "The Positions of Political Parties in Elections." in Niemi and Weisberg; Otto A. Davis and Melvin J. Hinich, "Spatial Competition Under Constrained Choice," in Niemi and Weisberg.

[31] That image, however, may be (instantaneously) altered by a dramatic act, e.g., a halt in the bombing of North Viet Nam in 1968.

[32] Stokes, "Spatial Models."

[33] Quite obviously, C.1 and C.2 are somewhat arbitrary. Moreover, in an historically dynamic world the ability of incumbents to appear equivocal renders C.1 overly restrictive. Attempts to relax them are given in Shepsle, *Essays . . .* , Chap. 3. For the purposes of this essay, however, they provide a well-specified electoral contingency suitable for analysis.

[34] Down's reliance on the *distribution of most-preferred points* in the electorate, and not on the functional form of utility schedules, prevented him from correctly specifying the possibilities of a strategy of ambiguity. For an early treatment of the importance of utility function form in decision making under risk and uncertainty, as well as some fascinating economic applications, see the classic paper by Milton Friedman and L. J. Savage, "The Utility Analysis of Choices Involving Risk," *Journal of Political Economy*, 56 (1948), 279–304.

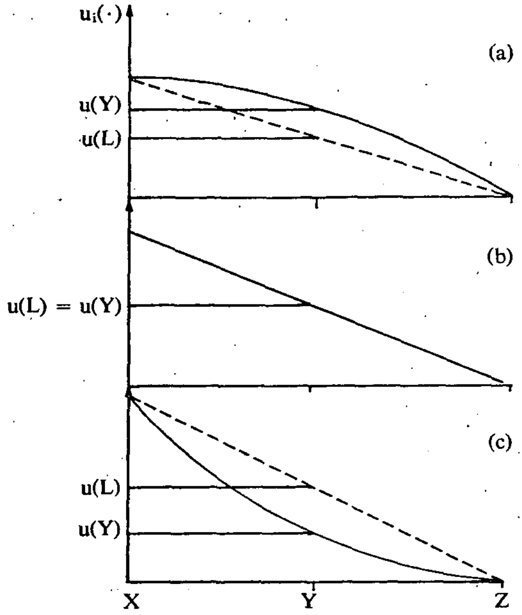

Figure 1. Voter utility functions.

Suppose now that these voters must choose between two policy alternatives:

(i) y for certain, or
(ii) $L=(1/2x,\ 1/2z)$, i.e., an ambiguous alternative assigning equal probability to x and z.

The utility of option (i) is determined directly from the utility functions displayed in Figure 1. The expected utility of L is $1/2\ u_i(x)+1/2\ u_i(z)$ and is represented by the midpoint of the chord (dotted lines in Figure 1) connecting the points $(x,\ u(x))$ and $(z,\ u(z))$ on the graphs of the utility functions. As is demonstrated in Figure 1, voter ***a*** prefers (i) to (ii), ***b*** is indifferent between the two, and ***c*** prefers (ii) to (i). In short, *despite identical preference orderings, each voter reacts differently to the ambiguous alternative depending upon the shape of his preference function.* Generally, voters like *a*, who possess concave preference functions, prefer the expectation of a gamble for certain to the gamble itself and are thus said to be *risk averse*. Voters like *b*, with linear preference functions, are indifferent between a gamble and the certainty of its expectation and are called *risk neutral*. Finally, voters with convex utility functions are, like *c*, *risk acceptant*, preferring a gamble to its expectation.[35]

The risk environment is characterized, secondly, by the sets of strategy constraints that govern candidate behavior. In particular, the distributional characteristics of available lottery strategies—domain and density—determine the viability of purposeful ambiguity. The domain of a given strategy, demarcating the region of the policy continuum over which the strategy is operative, is an important datum by virtue of our previous point on the functional form of preferences. If, as we shall see below, voters possess convex preferences over certain ranges of the policy continuum, and if the domain of strategies compatible with strategy constraints includes those same ranges, then ambiguity may be encouraged. In view of the central role it plays in the expected utility calculation, the probability density of any available strategies is also important.

Given these two characteristics, as well as the assumption of expected utility maximization and a well-defined electoral profile, the risk environment provides a suitable framework for examining electoral dynamics under uncertainty. Voters in our "game" somewhat mechanistically choose among the competing alternatives, some of which may be ambiguous, so as to maximize expected utility. *Candidates, on the other hand, may employ the risk environment (subject to strategy constraints) to their electoral advantage.*

Candidate Strategy Selection: Some Theoretical Implications[36]

Our task in this section is to derive some implications of A.1–A.7 and C.1–C.2 for candidate strategy selection and success in elections in which an incumbent candidate faces a challenger. With this arrangement,[37] in which candidate strat-

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[35] The expectation of a gamble $p(x)$, $\bar{p}$, is the expected value (not to be confused with the expected *utility*) of that gamble: $\bar{p}=\Sigma\, xp(x)$ for discrete probability distributions and $\bar{p}=\int xp(x)dx$ for continuous distributions. In the example above y is the expectation of $(\frac{1}{2}\ x,\ \frac{1}{2}\ z)$ by construction, i.e., it is the midpoint of $[x,\ z]$. The three voter types may be defined mathematically:

If	*then*
$u''(x) < 0$ (concavity)	$u(\bar{p}) > \Sigma\, u(x)p(x)$ (risk-averse)
$u''(x) = 0$ (linearity)	$u(\bar{p}) = \Sigma\, u(x)p(x)$ (risk-neutral)
$u''(x) > 0$ (convexity)	$u(\bar{p}) < \Sigma\, u(x)p(x)$ (risk-acceptant)

[36] For further results see Kenneth A. Shepsle, "Parties, Voters, and the Risk Environment," *passim.*

[37] Two other arrangements are possible in two-party competition. The first, in which two incumbents face each other (by virtue, say, of redistricting so that two elected officials, formerly representing distinct constituencies, now must compete with each other to represent a single constituency), is simply the original Downsian model in which constraint C.1 applies to both candidates. The second, in which two challengers face each other, is somewhat more complex. In this case, which might occur when an incumbent does not seek re-election or fails to obtain his party's nomination, and when his replacement is not considered his hand-picked successor, both candidates are governed by constraint C.2. This case is treated briefly in Shepsle, "Parties . . ." In a future paper, I expect to examine this case in greater detail.

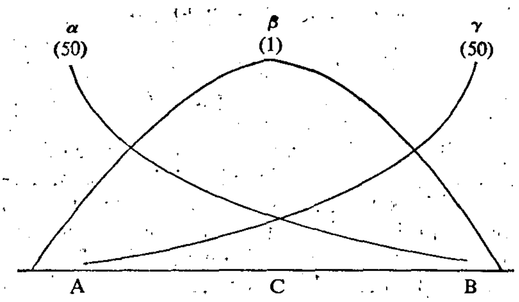

Figure 2. An electoral profile.

egy sets are defined by C.1 and C.2, respectively, it is of interest to identify the kinds of *electoral profiles* for which an electoral strategy in one candidate's strategy set is preferred by a majority of the voters to *all* strategies in his opponent's strategy set. We call an electoral strategy (or set of strategies) with this characteristic *winning*, and the contingency an *equilibrium situation*.[38]

It will be useful in this discussion to have a specific case in mind, although the results below are not restricted to this case. Suppose, then, that an electorate of 101 voters is to choose between two candidates according to their policy positions in [A, B]. The electoral profile is provided in Figure 2. Fifty voters possess type α preference functions, fifty type γ, and the single remaining voter type β. Assume that C is the midpoint of [A, B].

In earlier versions of the spatial model under contingencies of *certainty*, the well-known result of Duncan Black obtains:

> BLACK'S THEOREM: For an electorate composed of n voters (assume n odd), each of which has a single-peaked preference function arrayed along the issue dimension from most extreme in one direction to most extreme in the other, the policy position represented by the median most-preferred point receives at least a simple majority against every other point, and it is the only point that can do so.[39]

That is, the preferred point of the lone type β voter—the median most-preferred point—is the predicted position of the winning candidate. In fact, the median most-preferred point is a *winning strategy* in spatial competition under *certainty* governed by A.1–A.6 and C.1 (but not C.2).

The "drawing power" of the median is one of the more frequent and recurring themes of the spatial modeling literature. It is not difficult to show, however, for the electoral profile of Figure 2 and strategy constraints C.1 and C.2, that once uncertainty is permitted the median is no longer winning. Suppose, for example, that the incumbent governed by C.1, chooses the median most-preferred point (the point C in Figure 2). If the challenging candidate chooses the strategy:

$$\begin{aligned} p(x) &= 1/2 \qquad \text{for } x = A \text{ or } x = B \\ &= 0 \qquad \text{otherwise} \end{aligned}$$

a strategy consistent with C.2 then he will win a majority of votes. That is, the *ambiguous* strategy $p(x)$ defeats $x_{\rm med}$ (the median most-preferred point). To see this, consider the individual calculation made by each voter type. The type β voter, of course, prefers the incumbent, who is located at his "bliss point," to *any* lottery proposed by the challenger. The calculations of the type α and type γ voters, however, displayed in Figure 3, yield a different result. In Figure 3(a) the type α voters' evaluation of the risky candidate involves examining the midpoint of the chord connecting $(A, u(A))$ and $(B, u(B))$. Given the probability weights of 1/2 on the points A and B, the expected utility of $p(x)$ is:

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[38] The *nonuniqueness* of winning strategies poses no theoretical problems. The candidate simply chooses from among those strategies the one which maximizes votes (A.6). If two or more winning strategies are vote-maximizing, then any arbitrary standard may be employed for final selection. The question of *mathematical existence*, however, raises interesting theoretical possibilities to be explored below. Several observations are appropriate here. First, it is obvious that *both* candidates cannot possess winning strategies. Otherwise some majority would have to prefer $x^*\epsilon S_{\rm I}$ (the incumbent's alleged winning strategy), to all $y\epsilon S_{\rm II}$, on the one hand, while some other majority prefers $y^*\epsilon S_{\rm II}$ (the challenger's alleged winning strategy) to all $x\epsilon S_{\rm I}$, on the other. But this condition is contradictory, since any two majorities contain at least one person in common. Second, it is possible that *neither* candidate possesses a winning strategy. In this case, as in two-person zero-sum games without pure strategy saddle points, a problem in conjectural variation arises. Although there are optimal mixed strategies (technically, ϵ-optimal since this is an infinite game), this will not be our concern. In this essay the concern is the set of conditions under which *winning strategies* exist. In the last section of the paper the "non-existence problem" is addressed. There the relationship between winning strategies and electoral equilibrium is considered. On the relationship between spatial models and the theory of infinite games, the reader may consult Shepsle, *Essays . . .*, Chap. 4; and Hinich, Ledyard, and Ordeshook, "A Theory of Electoral Equilibrium." For an examination of social choice situations with ambiguous alternatives in which no winning strategy exists, see Richard Zeckhauser, "Majority Rule with Lotteries on Alternatives," *Quarterly Journal of Economics*, 83 (November 1969), 696–703; and Kenneth A. Shepsle, "Uncertainty and Electoral Competition: The Search for Equilibria," a paper presented at the Mathematical Collective Decisions Seminar, Hilton Head Island, South Carolina, August, 1971.

[39] This result is discussed in Black, *The Theory of Committees and Elections*, pp. 14–18. This discussion parallels that of Downs, *Economic Theory of Democracy*, Chap. 8.

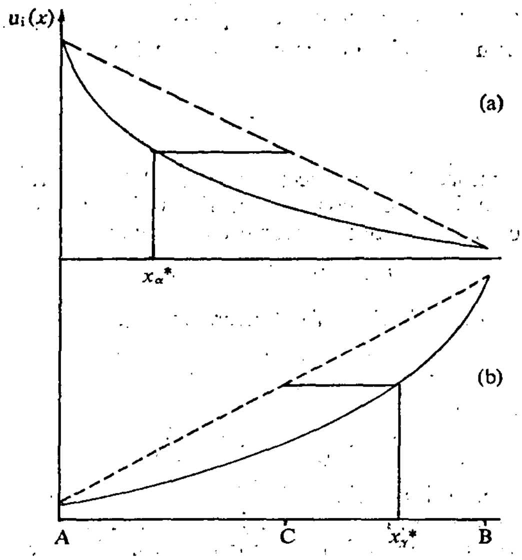

Figure 3. Lottery evaluations.

$$u_\alpha[p(x)] = 1/2u_\alpha(A) + 1/2u_\alpha(B),$$

which is the midpoint of the chord. But

$$u_\alpha[p(x)] = u_\alpha(x_\alpha^*),$$

where x_α^* is the *certainty equivalent* of the lottery $p(x)$,[40] and $u_\alpha(x_\alpha^*) > u_\alpha(C)$ by A.2 and A.3. Thus, by transitivity $u_\alpha[p(x)] > u_\alpha(C)$ and the fifty type α voters prefer the risky challenger. Similarly (see Figure 3(b)), the fifty type γ voters prefer the challenger. *The result is that the median is defeated by the vote of 100–1!* The important point, of course, is that Black's theorem does *not* generalize to all contingencies of uncertainty, and, moreover, that there may indeed be incentives for the rational candidate to "cloud his policies in a fog of ambiguity." Shortly this result is stated and proved formally. First, however, some theorems concerning conditions for which Black's theorem generalizes to contingencies of uncertainty are proved.

In essence, Black's theorem demonstrates that x_{med} defeats all *degenerate risks*, i.e., all certain alternatives. But it has even greater generality:

THEOREM 1: If *all* voters are risk averse (and hence possess concave preference functions in $[A, B]$), then x_{med} defeats any risk, degenerate or non-degenerate.

The proof is straightforward. We must show, for any $p(x)$, that x_{med} is preferred to it by a majority. Consider three mutually exclusive, collectively exhaustive classes of $p(x)$

$$\text{(i)} \quad E[p(x)] = \int_A^B xp(x)dx < x_{\text{med}}$$

$$\text{(ii)} \quad E[p(x)] > x_{\text{med}}$$

$$\text{(iii)} \quad E[p(x)] = x_{\text{med}}$$

For the first class choose one of the voters v_i whose most-preferred point $x_i \geq x_{\text{med}}$. By the assumption of risk-aversion, $u[p(x)] < u(E[p(x)])$. But $E[p(x)] < x_{\text{med}}$ by construction, and, from A.2 and A.3,

$$u(E[p(x)]) < u(x_{\text{med}})$$

(since in this region v_i's preference function is downward sloping to the left). By transitivity, then, $u[p(x)] < u(x_{\text{med}})$, and v_i prefers x_{med} to $p(x)$. But v_i is an arbitrarily selected voter from among those for whom $x_i \geq x_{\text{med}}$, and this collection of voters constitutes a majority by definition of the median. Therefore, a majority prefers x_{med} to any $p(x)$ in class (i). By analogous reasoning, a majority prefers x_{med} to any $p(x)$ in class (ii). To complete the proof, one need only observe that, since all voters are risk averse, all voters prefer the expectation $E[p(x)]$ to the risk, $p(x)$, itself, i.e., $u(E[p(x)]) > u[p(x)]$ for all v_i. In class (iii) $E[p(x)] = x_{\text{med}}$. Therefore, for all voters, $u(x_{\text{med}}) > u[p(x)]$ for $p(x)$ in class (iii). Thus, we have shown that the median is preferred to all $p(x)$, given the premise of the theorem.

In order to give serious attention to the substantive implications of Theorem 1, a meaningful political interpretation of risk aversion is required. While this would take us too far afield,[41] several brief comments are appropriate. If by risk aversion one has in mind an individual preference structure characterized as low-keyed, i.e., an unwillingness by individuals to "take a chance" on achieving a more preferred outcome when one can "settle" for a middle-ranked alternative, then such a situation as is presumed in Theorem 1 yields a *winning* strategy for the incumbent. That is, an *equilibrium situation* obtains when the issue being contested in the election is one on which preferences are not intense.[42] Thus, for electoral profiles involving nonintense preferences, there is a simple explanation for the electoral dominance of incumbent officials.[43]

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[40] For technical definitions, see John W. Pratt, Howard Raiffa, and Robert Schlaifer, "The Foundations of Decision Under Uncertainty: An Elementary Exposition," *Journal of the American Statistics Association,* 59 (1964), 353–375.

[41] What is required here is a serious discussion of the "units problem." This problem is usually avoided in the spatial modeling literature, but when the *shape* of preference functions is under consideration, it is particularly important to have the units of the strategy space specified. See Shepsle, "Uncertainty and Electoral Competition."

[42] For a mathematical definition of intensity based on reactions to risk, see Alvin Rabushka and Kenneth A. Shepsle, *Politics in Plural Societies: A Theory of Democratic Instability* (Columbus: Charles E. Merrill, 1972), Chap. 2.

Closely related to the existence of a winning strategy is the importance of *commitment* in risk environments permitting ambiguity.[44] For the class of issues on which voters are risk averse, the incumbent need not discredit his opponent's position. He need only discredit the *certainty* of that position. Where the premises of Theorem 1 are satisfied, vagueness is as costly an electoral posture as nonoptimal policy selection.

Finally, it may be noted that the electorate found in the early work of Davis and Hinich conforms to the premises of Theorem 1.[45] Their electorate is composed of voters with quadratic loss functions which, in terms of preferences, yields concave utility functions. Thus, the implication of the theorem applies to their work as well.

The assumption that *all* voters in Theorem 1 are risk averse is quite restrictive. Any weakening of that assumption, however, requires a specification of the distribution of risk-averse voters in order to provide determinate results. Since we would like our theorems to be independent of distribution assumptions (which otherwise restrict the generality of the results), we finesse the problem with a less determinate result:

THEOREM 2: If a *majority* of voters is risk averse, then the challenger possesses no winning strategy.

Theorem 2 asserts that for any electorate with at least a simple majority of risk-averse voters, there exists no strategy in the challenger's strategy set, S_{II}, which dominates all strategies in the incumbent's strategy set, S_I. Alternatively, it states that for any $p(x) \epsilon S_{II}$, there exists a $y \epsilon S_I$ which is preferred by at least a simple majority.

The proof of this theorem follows directly from the definition of risk aversion. For any $p(x) \epsilon S_{II}$ (recall that S_{II} is the set of strategies consistent with C.2), choose the point $y \epsilon S_I$, where

$$y = \int_A^B x p(x) dx.$$

That is, y is the expectation of $p(x)$. Since $[A, B]$ is a bounded continuum, the point y always exists. For any risk-averse voter, say v_i,

$$u_i[p(x)] < u_i(E[p(x)])$$

by the definition of risk aversion. But

$$y = E[p(x)], \text{ by construction,}$$

so that

$$u_i(y) > u_i[p(x)].$$

Since v_i is an arbitrarily selected risk-averse voter, and since there are a majority of such voters by assumption, it follows that y defeats $p(x)$. Moreover, since $p(x)$ is any strategy in S_{II}, there exists some $y \epsilon S_I$ that defeats *any* $p(x) \epsilon S_{II}$. This completes the proof.

It should be emphasized here that Theorem 2 says nothing about the existence of a winning strategy for the incumbent. In fact, in the absence of assumptions about the distribution of risk-averse voters, it is impossible to say whether an equilibrium situation obtains. This is somewhat surprising in light of the interpretation ordinarily given to single peakedness, namely that there always exists a winning strategy.

To this point, then, Black's theorem has been generalized to some contingencies of uncertainty, re-emphasizing "the power and importance" of the median most-preferred point (Theorem 1). Moreover, under somewhat weaker conditions, it has been shown that constraint C.2 implies that S_{II} contains no winning strategy, thus placing the challenger at a strategic disadvantage. However, at the same time we have called into question the "existence" part of Black's theorem, suggesting that there may well be electoral profiles for which *no winning strategy exists for either candidate*. In the next section this point is pursued in more detail. Before addressing ourselves to that question, we prove one final result, a generalization of the example with which this section was introduced.

Theorems 1 and 2, as well as the commentary that followed, apply to risk-averse electoral profiles only. Once the restriction of risk-aversion is removed, an entirely different picture emerges:

THEOREM 3: If a *majority* of voters is risk acceptant in some interval $I \subseteq [A, B]$, containing x_{med}, then any $p(x)$ defined on I with expectation at

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[43] As well, we may now assess the probable success of campaign rhetoric that encourages the electorate to resist changing horses in midstream: it is successful when voters are not risk-acceptant on the issue in question.

[44] On the importance of commitment in more general strategic situations, see Thomas C. Schelling, *The Strategy of Conflict* (New York: Oxford University Press, 1963), *passim.*

[45] Otto A. Davis and Melvin J. Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in *Mathematical Applications* . . . J. Bernd, ed., Vol. II, pp. 175–208; "Some Results Related to a Mathematical Model of Policy Formation in a Democratic Society," in Bernd, Vol. III, pp. 14–38; "On the Power and Importance of the Mean Preference in a Mathematical Model of Democratic Choice," *Public Choice,* 5 (Fall 1968), 59–72.

x_{med} defeats the median most-preferred point.

To see that Theorem 3 is true, choose a $p(x)$ over an interval $I \subseteq [A, B]$ such that

(i) $x_{\text{med}} \in I$, and

(ii) the expectation of $p(x)$ is x_{med}, i.e.,

$$E[p(x)] = \int_I xp(x)dx = x_{\text{med}}.$$

It is clear that one can choose a $p(x)$ in this fashion since a uniform probability distribution (over an interval containing x_{med} and symmetric about that point) satisfies (i) and (ii) above. Now choose a voter, v_i, who is risk-acceptant. By the definition of risk acceptance,

$$\int_I u_i(x)p(x)dx > u_i(E[p(x)]).$$

But, by construction, $E[p(x)] = x_{\text{med}}$. Thus,

$$\int_I u_i(x)p(x)dx > u_i(x_{\text{med}}).$$

That is, v_i prefers $p(x)$ to x_{med}. Since v_i is an arbitrarily selected risk-acceptant voter, and since a majority of voters is risk-acceptant, it follows that the last inequality must hold for at least a simple majority. Thus, $p(x)$ defeats x_{med}, and the theorem is proved.

A situation conforming to the premises of Theorem 3 is displayed in Figure 2. The type α and type γ voters—a majority in all—are risk acceptant over the entire interval $[A, B]$. Thus, there are strategies in S_{II}, one of which was given in the example, that defeat x_{med}.

Theorem 3 is significant. It should disabuse the reader of the universal superiority of the median. Elsewhere I have argued that the premises of the theorem provide a convenient definition of intensity of preference, i.e., a risk-acceptant voter is one willing to "take a chance" on obtaining his most-preferred alternative, even at some rather large risk of obtaining considerably worse alternatives, rather than to settle for a "middling" alternative.[46] With this interpretation, Theorem 3 asserts that the incumbent finds his position tenuous on those issues which polarize the electorate into groups with intense, conflicting preferences. *It is precisely these kinds of "critical issues," then, which encourage deliberate ambiguity by candidates.*

Theorem 3, like Theorem 2, does not imply the existence of a *winning strategy* or an equilibrium situation. In particular, though we know that $p(x) \epsilon S_{II}$ exist that defeat x_{med}, we do not know whether any $p(x)$ defeat *all* $x \epsilon S_I$.[47] It is to this important question that we now turn our attention.

The Search for Equilibrium

Given the last theorem, suppose that for each $p(x)$ that defeats x_{med} some other point $x^* \epsilon [A, B]$ exists that defeats $p(x)$. That is,

(i) $p(x)$ defeats x_{med} (Theorem 3)
(ii) x^* defeats $p(x)$ (by construction)
(iii) x_{med} defeats x^* (Black's Theorem)

Assertion (i) simply restates the conclusion of Theorem 3 (here, by assumption, the premises are met). Assertion (ii) is a supposition. Assertion (iii) draws on Black's Theorem and the property of the median most-preferred point. But assertions (i) – (iii) jointly yield a collective intransitivity—the so-called cyclical majority problem. *Thus, if an $x^* \epsilon [A, B]$ satisfying (ii) exists, then there are no winning strategies and no equilibrium situations.* That such an x^* can exist seems intuitively plausible. In this analysis, however, we must cast intuition aside and rely on more formal means.[48]

Since this implication (if it is true) is important, it is reiterated. If it is the case that whenever challenging elites perceive incentives to equivocate in their electoral campaigning (i.e., whenever the premises of Theorem 3 are met) that the incumbent possesses a counterstrategy in his strategy set, then it follows that no equilibrium situation exists. The consequences for spatial models of electoral competition in various risk environments, then, are considerable, namely the search for equilibrium will be in vain—none exists!

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[46] See Kenneth A. Shepsle, "Intensity, Salience, and the Risk Environment: Some Theoretical Aspects of Conflict in the Plural Society," a paper presented at the Conference on Theories of Collective Behavior, Fels Center of Government, University of Pennsylvania, December, 1970. For an empirical application of this notion of intensity, see Rabushka and Shepsle, *Politics in Plural Societies.*

[47] Note that the median-defeating strategies $p(x) \epsilon S_{II}$ need not be unique. This lack of uniqueness, however, is inconsequential for our argument.

[48] Since $x_{med} \epsilon S_{II}$, strategy x^* is not threatened by it, so that we do not technically have a contingency of collective intransitivity. But a median-defeating strategy $p^*(x) \epsilon S_{II}$ can be found, distributed entirely in an interval of length ϵ about x_{med}, for arbitrarily small ϵ. It may then be shown that $p^*(x)$ defeats x^*. If this $p^*(x)$ is invulnerable, then a collective intransitivity is averted. However, if for *any* median-defeating strategy in S_{II} there is an x^* which in turn defeats it, then a nonequilibrium situation is insured. It is of theoretical interest to specify the conditions under which these events transpire. Our intentions in what follows are more modest and these fundamental theoretical issues are left as open questions.

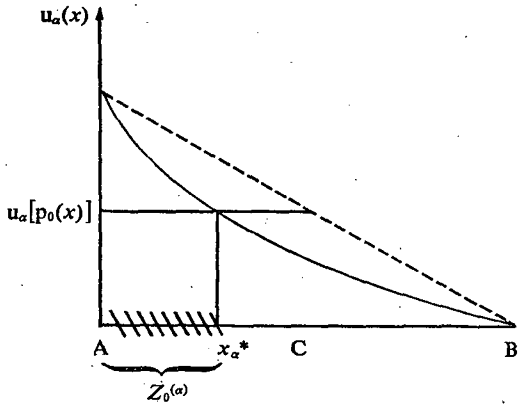

Figure 4. Interval of points preferred to the lottery.

For the case of a *finite* number of alternative strategies (as opposed to a continuum of strategies), a finding by Zeckhauser is important.[49] Zeckhauser has proved that if there are a *finite* number of basic alternatives (i.e., elements in S_T), if voter preference functions are single-peaked, if *lotteries* (rather than probability density functions) among basic alternatives are permitted, and if some lotteries are able to defeat the median alternative (Theorem 3), *then it is always the case that an x^* exists which defeats such lotteries*. That is, with finite alternatives, contingencies which provide incentives to equivocate (to choose ambiguous strategies) invariably yield collective intransitivities.

In order to trace the implications of Zeckhauser's Theorem for our model, a proposition for the bounded continuum (A.1) analogous to Zeckhauser's (which, to repeat, applies to the case of finitely-many alternatives) is constructed. It is then shown that this proposition, unlike Zeckhauser's Theorem, is *not* generally valid.[50]

Suppose the premises of Theorem 3 are met. Then median-defeating probability distributions exist. Write this collection of strategies as

$$P = \{p(x)/u_i[p(x)] > u_i(x_{\text{med}}) \text{ for at least a simple majority}\}.$$

Since Theorem 3 establishes the existence of median-defeating strategies, we know that P is nonempty, i.e., $P \neq \emptyset$. Thus we may select some particular probability distribution in P, say $p_0(x)$, and define

$$Z_0^{(i)} = \{x/u_i(x) > u_i[p_0(x)]\}.$$

$Z_0^{(i)}$ is the set of points in $[A, B]$ which are preferred by the i^{th} voter, v_i, to $p_0(x)$. For example, if $p_0(x)$ is the probability distribution over $[A, B]$ for the electoral profile in Figure 2, then $Z_0^{(\alpha)}$ for the type α voters is the interval $[A, x_\alpha^*)$, shown in Figure 4. Any point in $[A, x_\alpha^*)$ is preferred by a type α voter to $p_0(x)$. Generally speaking, $Z_0^{(i)}$ is an interval, a consequence of the nature of postulated preferences (A.2–A.3).

Since there are N voters $\{v_1, v_2, \cdots, v_N\}$, there are $\binom{N}{d} = m$ simple majority coalitions, where

$$d = \begin{cases} \dfrac{N+1}{2} & \text{if } N \text{ is odd} \\ N/2 + 1 & \text{if } N \text{ is even.} \end{cases}$$

We may arbitrarily label these minimum winning coalitions $c_1, c_2, \cdots, c_m$, and identify the collection of such coalitions by

$$C = \{c_1, c_2, \cdots, c_m\}.$$

Define $\bigcap_j Z_0^{(i)} = \{x/x\epsilon Z_0^{(i)} \text{ for each } v_i\epsilon c_j, c_j\epsilon C\}$. That is $\bigcap_j Z_0^{(i)}$ is the set of points common to the $Z_0^{(i)}$ of each v_i in the jth winning coalition. Clearly, if for any of the c_j, $\bigcap_j Z_0^{(i)}$ is nonempty, then there are points common to the $Z_0^{(i)}$ of at least a simple majority of voters. That is, there are points preferred by a majority to $p_0(x)$. Consequently, if nonempty $\bigcap_j Z_0^{(i)}$ exist for all $p_0(x)\epsilon P$, then no winning strategies exist and a cyclical majority problem is generated.

The generalization of Zeckhauser's Theorem may be stated:

PROPOSITION: There exist $c_j\epsilon C$

$$\text{such that } \bigcap_j Z_0^{(i)} \neq \emptyset.$$

To see that the proposition implies a nonequilibrium situation, it suffices to prove the following

CONSEQUENCE:

$$x_{\text{med}} \notin \bigcap_j Z_0^{(i)} \qquad \text{for any } c_j.$$

A cyclical majority problem follows immediately:

(i) $p_0(x)$ defeats x_{med} (Theorem 3)

(ii) $x^*(\neq x_{\text{med}})$ defeats $p_0(x)$,

where $x^*\epsilon\bigcap_j Z_0^{(i)}$ for some c_j (Definition of $Z_0^{(i)}$ and Consequence)

(iii) x_{med} defeats x^* (A.2, A.3, and Black's Theorem).[51]

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[49] Zeckhauser, "Majority Rule."

[50] Elsewhere I have examined Zeckhauser's Theorem for electoral profiles with nonsingle peaked preferences, proving that equilibrium situations in these contingencies may exist. See Kenneth A. Shepsle, "A Note on Zeckhauser's 'Majority Rule with Lotteries on Alternatives': The Case of the Paradox of Voting," *Quarterly Journal of Economics,* 84 (November 1970), 705–710; and "The Paradox of Voting and Uncertainty," in Niemi and Weisberg, forthcoming.

[51] See footnote 48.

Proof of the Consequence: Suppose $x_{\text{med}} \in \bigcap_j Z_0^{(i)}$ for some $c_j \in C$. Then, by definition of $\bigcap_j Z_0^{(i)}$, a simple majority prefers x_{med} to $p_0(x)$. But, by construction and Theorem 3, $p_0(x)$ is preferred by a simple majority to x_{med}. Since any two majorities contain at least one person in common (this is easily shown),[52] that person simultaneously prefers $p_0(x)$ to x_{med} and x_{med} to $p_0(x)$. Contradiction! Thus, $x_{\text{med}} \notin \bigcap_j Z_0^{(i)}$ and the Consequence is established.

With this Consequence, then, we can be assured that if the Proposition is valid, the search for equilibrium will be an unrewarding one.

For a detailed treatment of the logical status of this Proposition, the reader is referred elsewhere.[53] For our purposes it only need be reported that some electoral profiles can be constructed for which the Proposition is true, others for which it is false. *Thus, the Proposition is neither generally true nor generally false.* That is, electoral profiles exist, on the one hand, for which the Proposition and concommitant disequilibrium obtain. On the other, electoral profiles exist for which the Proposition fails to hold. In these latter situations, the challenger possesses a strategy, consistent with C.2, which is *winning.*

Although this result may be disconcerting to the spatial modeler, it sheds some important light on the problems of electoral stability. In particular, it forces us to be attentive to the nature of *electoral profiles*, as defined earlier, rather than solely to the form of voter distribution, typically the concern of the spatial modeler.[54] Moreover, the result suggests that electoral instability, *even granting the rather restrictive assumption of single-peakedness*, is a sufficiently likely theoretical possibility that is deserving of our attention. Indeed, it may be the rule rather than the exception! It is to these points, as well as a summary of what has preceded, that we turn in the concluding section of this essay.

Conclusions

Anthony Downs has argued that (1) since "ambiguity . . . increases the number of voters to whom a party may appeal," (2) since "this fact encourages parties to be as equivocal as possible about their stands on each controversial issue," and (3) since "this makes it more difficult for each citizen to vote rationally," it follows that "rational behavior by political parties tends to discourage rational behavior by voters."[55] Our analysis has restricted the validity of the premises of Downs's argument. Thus, the conclusion is not generally valid.

First, it was demonstrated in Theorems 1 and 2 that ambiguity actually decreases the appeal of a candidate, that a candidate restricted to ambiguous strategies is positively disadvantaged, and that in the contingencies presumed in the theorems it is *irrational* for candidates not so constrained to behave equivocally. That is, if all voters are risk-averse, an equivocal challenger is vulnerable to a median-adopting, nonequivocating incumbent. Moreover, if only a majority of voters is risk-averse, a challenger possesses no winning strategy. Indeed, the overriding strategic problems in contingencies of risk aversion are those of commitment and credibility, not equivocation.

Second, it was shown in Theorem 3 that, *for a different electoral profile*, ambiguity and equivocation may be positively rewarding. The effect of this theorem is a limitation of the scope for which Downs's premises are valid. Specifically, if a majority of voters is risk-acceptant (and, by interpretation, possesses intense preferences, thus rendering the issue "critical"), then parties may be "encouraged to be as equivocal as possible about their stands . . . "

The "may be" in the assertions above needs to be underscored, however, for, third, we have shown through a generalized version of Zeckhauser's Theorem that the dominance of an ambiguous strategy cannot be determined without a further specification of the electoral profile.[56]

Fourth, Downs's third premise above, which derives from an unnecessarily narrow definition of rationality, is called into question. If uncertainty renders rationality problematic, then the rational decision maker is an empirical myth since, empirically, decisions are never made under conditions of perfect certainty. Thus, Downs's inference about the effects of uncertainty on rationality, leads to one of two actions:

(1) eliminate the rational man concept entirely, since uncertainty is pervasive in any empirical setting, or

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[52] See John G. Kemeny, J. Laurie Snell, and Gerald L. Thompson, *Introduction to Finite Mathematics*, 2nd ed. (New York: Prentice-Hall, 1966), p. 82.

[53] In particular, see Shepsle, "Uncertainty and Electoral Competition . . ."

[54] See Downs, *Economic Theory of Democracy*, Chap. 8, and Davis, Hinich, and Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process."

[55] Downs, p. 136.

[56] In "Parties, Voters, and the Risk Environment . . . ," I conjectured that under the conditions of THEOREM 3 there does exist a winning strategy in the challenger's strategy set. However, the more recent generalized version of Zeckhauser's Theorem makes that conjecture questionable, or at least incompletely specified. For a recent rather general mathematical treatment of the question of social choice under conditions of uncertainty, see Peter Fishburn, "Lotteries and Social Choice," a paper presented at the Mathematical Collective Decisions Seminar, Hilton Head Island, South Carolina, August, 1971.

(2) provide a different interpretation for the concept of rationality.

I have opted for the latter alternative, arguing that rationality simply implies the efficient use of contextual information so as to produce actions consistent (*a priori*) with preferences. The expected utility maximization rule, our rule of voter decision making, is compatible with this view.

Finally, the theoretical appropriateness of searching for equilibrium situations when disequilibrium may be the theoretically interesting state was questioned. In the derivation of the generalized version of Zeckhauser's Theorem, and in the claim that examples exist which render this Proposition both true and false (but not simultaneously, of course!), it was suggested that the "search for equilibria" may not be the appropriate theoretical posture—that instability and the absence of equilibrium (in the dominance sense) may be the rule rather than the exception, and that the interesting behavior to model is the strategic adaptation of actors to these nonequilibrating contingencies.

The particular way in which uncertainty was introduced in the context of spatial models of electoral competition may be inadequate, and is most certainly incomplete. Not included, for example, are any manifestations of uncertainty in candidate decision making except as they arise in the game-context of strategy selection. Thus, it was supposed that there is no uncertainty in candidate information about voter preferences or strategy constraints.[57] It would be of great interest to incorporate questions of this sort. Moreover, it should be emphasized that the results in this paper depend upon highly restrictive assumptions. Increased mathematical generality, then, is a first-order concern.

Caveats aside, the results derived in the body of this paper suggest, minimally, that the risk environment is not neutral. And, along with the electoral profile and strategy constraints, the risk environment is conceptually useful in providing nonobvious insights about the strategic behavior of political elites.

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[57] For an interesting essay that deals with candidate uncertainty about the parameters of the voter distribution and the effect of this on the decision to allocate resources for information collection, see Jane Gilbert, "Who Gets Information, Where, and When" (University of Rochester, 1970, unpublished manuscript).

Cluster-Bloc Analysis and Statistical Inference*

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Introduction

The analysis of legislative voting and judicial decisions has long been regarded as a proper domain of study for the political scientist. But, because of the vast amount of work involved, few systematic studies have emerged. In addition the prospective researcher must choose from at least five methodological approaches: (a) the construction of indices of cohesion for particular groups, (b) cluster-bloc analysis, (c) Guttmann scaling, (d) factor analysis, and (e) "complete analysis." The main use of all the methods is to provide a description of the structure of the various groups in the legislature. Guttmann scaling is the only method in which the voting groups must be described with reference to a particular issue set of related roll calls; the other methods may be used with either a set of related roll calls or with *all* the issues on which votes were taken.[1]

Hovet has examined the voting records of the United Nations General Assembly and its main committees for the first sixteen UN Sessions.[2] He was concerned to discover how cohesive the various groups, that meet behind the scenes in caucus, actually are in their roll-call voting. For each group he constructed percentage indices of identical voting, "solidarity" voting (when the dissenting members abstain rather than vote against the group), and divided voting. Lijphart has shown that these indices are not sound for comparing groups of ten or less members.[3] Lijphart also suggested that we can understand the voting structure of the UN much better when we empirically determine the voting blocs than when we examine the record of predetermined groupings. Lijphart suggested that cluster-bloc analysis, as developed by Beyle in 1931 from earlier work by Rice in the 1920s, was a more satisfactory methodology.

The three other alternative methods also allow the groups to emerge empirically from the voting records. Guttmann scaling is well known and is conceptually simple to understand. But for the analysis of UN voting, which is the author's main interest, it has only been used to examine one UN Session,[4] although it has been used more extensively to study other bodies. Guttmann scaling is not necessarily an alternative to cluster-bloc analysis but can serve as a useful complement to it. Once blocs have been discovered, we can then see whether they can be described in terms of the issues which unite them. We can ask if the bloc has a low variance in its scores on a Guttmann scale.

The two remaining methods, factor analysis[5] and "complete analysis,"[6] have the advantage that they are firmly based on well developed statistical methods. Many political scientists, however, remain skeptical about applying such high-powered methods to simple YES, NO, ABSTAIN data.[7] What is substantively more im-

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[*] Thanks are due to Dr. C. Powell (Strathclyde) and Professor A. Lijphart (Leiden) for helpful discussion during the formulation of these ideas, to Professor P. Russell (Toronto) and Dr. D. Mann (Makerere) for comments on the first draft, and to P. Fotheringham (Glasgow) for the data cards used to derive Tables 3 and 4.

[1] In everyday use the words "vote," "roll call" and "issue" may be used with a high degree of interchangeability. To avoid confusion in this article, the use of the nouns will be restricted as follows: "vote"—one decision of Yes/No/Abstain by one individual; "roll call"—the aggregate result of all the members publicly voting on one resolution, bill, amendment, case or procedural question; "issue"—an underlying dimension that is common to a set of related roll calls.

[2] Thomas Hovet, *Bloc Politics in the United Nations* (Cambridge, Mass.: Harvard University Press, 1960); Thomas Hovet, *Africa in the United Nations* (Evanston, Ill.: Northwestern University Press, 1963).

[3] Arend Lijphart, "The Analysis of Bloc Voting in the General Assembly: A Critique and a Proposal," *American Political Science Review*, 57 (December 1963), 902–917.

[4] Leroy N. Rieselbach, "Quantitative Techniques for Studying Voting Behavior in the UN General Assembly," *International Organization*, 14, (Spring 1960), 291–306.

[5] On UN voting, Hayward R. Alker and Bruce M. Russett, in *World Politics in the General Assembly* (New Haven: Yale University Press, 1965), used factor analysis to discover issue dimensions and described states in terms of factor scores. Bruce M. Russett, in "Discovering Voting Groups in the United Nations," *American Political Science Review* 60 (June 1966), 327–339, used the alternative Q-type factor analysis.

[6] Keith Hope, "Complete Analysis: A Method of Interpreting Multivariate Data," *Journal of Market Research Society* (U.K.) 2 (1969), 267–284. The example given in this article used data from the 17th session of the UN.

[7] John E. Mueller, "Some Comments on Russett's 'Discovering Voting Groups in the United Nations'" *American Political Science Review*, 51 (March 1967), 146–148, Oran R. Young, "Professor Russett: Industrious Tailor to a Naked Emperor," *World Politics*, 21 (April 1969), 486–495, Robert E. Riggs, Karen F. Hanson, Mary Heinz, Barry B. Hughes and

portant is that, although these methods may successfully discover voting blocs, they tell us very little about the relationships *between* blocs. As Truman puts it, cluster-bloc analysis "has the considerable advantages of keeping close to the data and of requiring few assumptions in manipulating them."[8] For his study of the 81st Congress, Truman observed that the method offered other advantages. "Given the availability of the paired agreement scores, it was possible to perform a number of specialized analyses for particular subgroups within the parties . . . such as the seniority leaders, the elective leaders, the policy committees and the like."[9] Truman comments that in using this method "one is obliged to pay a price, namely foregoing most of the refined techniques of statistical inference that are at least theoretically possible with more elaborate methods."[10] The aim of this paper is to show that it is not necessary to pay such a price. The cluster blocs need only show paired agreement scores that are statistically acceptable at stipulated level of significance.

The Concept of Random Voting

The first step in cluster-bloc analysis is to calculate an agreement score for every possible pair of members of the legislative body. Then these scores must be scanned to see whether they qualify for inclusion in a table or a matrix or a diagram, (depending on the researcher's preference), to illustrate those groups of members that together form a cluster bloc. The resulting bloc is thus a subgroup of the legislature that shows greater than average cohesion. Every member of the bloc is in high agreement with every other member of the bloc. The problem is what constitutes "high agreement"? The following quotations (with italics added) show how this problem has worried all those who have used cluster-bloc analysis. Truman in his study, *The Congressional Party*, said that

> The choice of this cut-off point is an *arbitrary one* . . . Nevertheless, the choice need not be haphazard. If it is set high the blocs will be relatively small . . . But the number of votes on which all members of the bloc are agreed will be relatively high. If it is set low, the blocs will be larger . . . but the number of unanimous votes within the blocs will be smaller. . . . In the Senate sets [of roll calls] agreements on more than half of the roll calls were considered.[11]

Lijphart faced the problem in studying UN voting:

> The figure can be *arbitrarily set* at 95, 90, or 80 per cent for instance . . . it is preferable to analyze the Indices of Agreement at successively lower levels, beginning with Indices of 100 per cent, in order to discover the exact level at which the blocs emerged.[12]

Schubert was concerned with the analysis of judicial decisions:

> *On the basis of limited empirical application* . . . for the tables of overall inter-agreement, which are expressed in percentages, the average of the ratios of the included pairs can be used as an Index of Inter-Agreement, with .70 or better considered high, .60 to .69 moderate and less than .60 low.[13]

As long ago as 1931, Beyle tried to tackle this problem of determining a significant level of agreement. He started by considering what would happen if the legislators were voting purely at random and pointed out that

> If only chance factors were operative, there would be a 50-50 chance of a legislator voting on one side of a question, one half of unity. Similarly, the chance of a second legislator so voting would be also one half of unity. The chance that both legislators would so vote, and thus be in agreement, would be one-half times one-half, or one fourth of unity.

Up to this point Beyle is entirely correct in his reasoning. He goes on:

> Given 8 votes . . . their chance agreement on a given side of the questions would be one fourth of eight or two agreements. [An index may be constructed such that] the numbers between two and eight would be represented by proportionate degrees between zero and 100. Thus, the index number representing the significance of three agreements would be 16.6; the index number for four agreements would be 33.3; for 5, 50.0; for 6, 66.6; and for 7, 83.3. Index numbers for 33.3 may be taken to be particularly significant as indicating cohesion equal to, or greater than two times chance.[14]

Hayes pointed out in 1938 that this index has no meaning or value. "The *degree* of relation-

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Thomas J. Volgy, "Behavioralism in the study of The United Nations," *World Politics* 22 (January 1970), 192–326, see especially p. 211.

[8] David Truman, *The Congressional Party* (Wiley, New York, 1959), p. 329.

[9] Truman, p. 326.

[10] Truman, pp. 321–322.

[11] Truman, p. 324.

[12] Arend Lijphart, "Analysis of Bloc Voting . . . ," p. 913.

[13] Glendon A. Schubert, "The Study of Judicial Decision-Making as an Aspect of Political Behavior," *American Political Science Review*, 52 (December 1958), 1007–1025: quotation from pages 1012–13. (There is actually a substantial text covered by the ellipsis dots, but Schubert's meaning is not violated.)

[14] Herman C. Beyle, *Identification and Analysis of Attribute-Cluster-Blocs* (University of Chicago Press, 1931), 29–31.

ship and the *probability* of relationship are not at all the same thing. Degree of relationship is independent of the size of the sample involved, while probability is directly affected by the size of the sample."[15] Beyle is right that one would expect by chance to get two agreements on one side, out of eight roll calls, but he is wrong to say that twice the expected result is significant. Significance is not determined by the result being two, three, or any multiple of the expected value of a sampling distribution but by the result being so many standard deviations from the expected value (assuming normality).

Unfortunately, Hayes himself then pursued a completely fallacious argument; he attacked Beyle's assumption that random voting was the proper standard of comparison, claiming that for two legislators

> the probability of their agreements being within the possible range of chance factors depends upon the frequency with which each of them votes "aye" and "nay" . . . Only when given those *frequencies* can one calculate the probability of their agreeing with each other. For instance if two legislators each voted "aye" on every bill that came up, of course they would *agree* on every bill. However, that would be no indication that they were significantly related (in statistical terminology) to each other.[16]

Intuitively this conclusion makes no sense. If two legislators are in 100 per cent agreement on enough roll calls, there *must* be common political factors at work. Either they cooperate, or they are both loyal to the majority leader, or they both believe in following a Congressional consensus, or some other political process is operating. No person can be regarded as having an innate probability of voting "aye" of anything other than 0.5, unless he is motivated by some factor such as a desire to follow the consensus. Such a discovery would be in itself of interest and be politically significant.

As a result, Beyle's basic tenet must be upheld: an agreement score must be regarded as significantly high if it is larger than any value that would be likely to occur with random voting. Beyle was only concerned with agreement occurring on one side of a question. Most people now use indices of identical voting that count joint agreement either for *or* against the question; such indices will be discussed in the remainder of this paper.

If two legislators A and B, both vote at random on a roll call, there are four possible outcomes to the voting:

	A's vote	*B's vote*	
1)	Yes	Yes	—Identical voting
2)	No	No	
3)	Yes	No	—Disagreement
4)	No	Yes	

With the probability of voting Yes equal to the probability of voting No and both equal to 0.5, the probability of each result is $0.5 \times 0.5 = 0.25$. The probability of agreement is then the probability of occurrence of either the first outcome or the second outcome, i.e., $0.25 + 0.25 = 0.5$. Similarly the probability of disagreement is 0.5, for any one roll call. Having started by considering just one legislator voting Yes or No, we have now moved one step further and derived the probability of agreement for a pair of legislators jointly voting on the same roll call. The symbol p_f, will be used to signify the probability of agreement and p_d the probability of disagreement. Although the original assumption of random voting with $P(\text{Yes}) = P(\text{No}) = 0.5$ leads to a conclusion with a similar result, namely $p_f = p_d = 0.5$, it must be remembered that we are now dealing with the joint behavior of a pair of legislators and not with an individual legislator.

What happens if the two legislators each vote on eight separate roll calls? There are nine possible *agreement* patterns. The two legislators may fail to agree on any of the eight roll calls, or they may agree on 1, 2, 3, . . . or even all eight roll calls. This gives nine possible agreement scores, ranging from 0 per cent to 100 per cent. The probability of each score's occurring by random voting can be calculated using the general laws of probabilities and the binominal distribution.[17]

P(r agreements in N roll calls)

= No. of ways of getting r agreements times Probability for each way.

$$= C^N_r p_f^r p_d^{N-r}$$

As emphasized above, the "number of ways of getting r agreements" concerns the joint behav-

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[15] Samuel P. Hayes, "Probability and Beyle's 'Index of Cohesion'," *Journal of Social Psychology*, 9 (1939), 161–167; quotation from p. 161, italics in the original.

[16] Hayes, p. 163, italics in the original.

[17] A discussion of probability theory and the binominal distribution is given in most statistical textbooks. See for example Blalock, *Social Statistics* (New York: McGraw-Hill, 1960) for a particularly clear exposition. The argument at this stage is identical to the conventional discussion of how many heads will be expected in N tosses of a coin.

Table 1. Probability of Obtaining Each Agreement Score, with Eight Roll Calls

1	2	3	4	5	6
No. of Agreements	% Score	Probability of each Score		Cumulated Probabilities	
0	0%	$C^8_0 \quad p_f^0 \quad p_d^8$	.00391	.00391	1.00000
1	12.5%	$C^8_1 \quad p_f^1 \quad p_d^7$	.03125	.03516	.99609
2	25.0%	$C^8_2 \quad p_f^2 \quad p_d^6$	.10937	.14453	.96484
3	37.5%	$C^8_3 \quad p_f^3 \quad p_d^5$	.21875	.36328	.85547
4	50.0%	$C^8_4 \quad p_f^4 \quad p_d^4$	.27344	.63672	.63672
5	62.5%	$C^8_5 \quad p_f^5 \quad p_d^3$	.21875	.85547	.36328
6	75.0%	$C^8_6 \quad p_f^6 \quad p_d^2$	.10937	.96484	.14453
7	87.5%	$C^8_7 \quad p_f^7 \quad p_d^1$	.03125	.99609	.03516
8	100.0%	$C^8_8 \quad p_f^8 \quad p_d^0$	.00391	1.00000	.00391

ior of two legislators. Thus there is only "one way" of getting no agreements at all: they are opposed to each other on every one of the eight roll calls.[18]

The probability equations for 8 roll calls have been set up in column 3 of Table 1. The C^N_r term is evaluated according to the formula:

$$C^N_r = \frac{N!}{r!(N-r)!}$$

We have already seen that $p_f = 0.5$ and also that $p_d = 0.5$. When these values are substituted in the expressions given in column 3 of Table 1, the results obtained are in column 4. When the probabilities for the nine possible agreement scores are summed, the total comes, as it always must, to 1.0. Figure 1 shows that, even in voting on as few as eight roll calls, the sampling distribution approaches a normal curve.

The values of the agreement scores, that are significantly different from the "expected" mean value 50 per cent, are found by examining the tails of the sampling distributions. At the .001 level of significance, with eight roll calls no score is significant. Even 100 per cent agreement could occur by chance as often as four times in a thousand. But, using a single-tail test, 100 per cent is significant at the .01 level. When we move up to the .025 level of significance 100 per cent is still the only significant score. By the time we consider a .05 level of significance and are prepared to take the greater risk of being wrong in our inference, a score of 87.5 per cent agreement may also be considered significant. Thus in performing cluster-bloc analysis on eight roll calls the cutoff points for forming blocs should be either 87.5

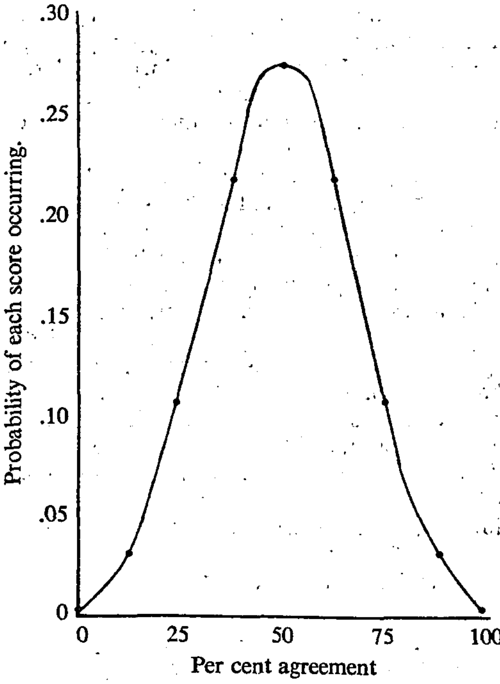

Figure 1. Sampling distribution for random voting. 8 roll-calls, Simple Percentage Index of Agreement.

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[18] This result could occur if legislator A voted Yes on all roll calls and B voted No on all roll calls; or the reverse could occur; or A could vote Yes on the first four and No on the second four, with B voting the reverse way; etc. etc. These variations which all produce the same agreement score of 0 per cent already been taken into account by the derivation of p_f and p_d from the four original Yes-No voting patterns.

per cent or 100 per cent, depending on the level of significance at which the researcher wishes to operate.

As the sampling distribution is completely symmetrical we can also consider the second tail of the distribution and find scores so low that they represent *significant disagreement.* Thus, on eight roll calls, 0 per cent is significant at the .01 or .025 level, and 12.5 per cent or less is significant at the .05 level. To the author's knowledge no other researchers have considered particularly low scores in their analyses of voting patterns. In any large assembly it is difficult enough to handle all the information generated about high agreement, and in any case most assemblies have enough consensus operating that scores below 30 per cent or so would be unusual. If, however, an assembly were found to be divided into two or more distinct cluster blocs, it would be well worth considering whether the divisions are so deep that there is also significant disagreement between the blocs or between some of their members.

So far the discussion has only considered voting on eight roll calls. If a different number of roll calls were under consideration, then the whole argument must be repeated to find the cutoff points for the agreement scores on N roll calls. The general formula is that J or fewer agreements will occur on N roll calls with a probability of

$$P = \sum_{I=0}^{I=J} C_I^N \cdot p_f{}^I \cdot p_d{}^{N-I}$$

Fortunately this expression can be evaluated by computer. The results, for random voting on up to 250 roll calls, are given in Table 2. After 30 roll calls the results are only given for every 10, and after 80 roll calls for every 20, since the differences in cutoff points become very slight. To err on the side of caution, one should use the cutoff points higher in the table.[19] Thus, if 57 roll calls were being considered and the .05 level of significance were adopted, one should use the figure given for 50 roll calls (cutoff = 64.0 per cent) rather than that for 60 roll calls (cutoff = 61.7 per cent).

The Definition of Indices

We must now examine more carefully the assumption that random voting is the proper standard of comparison for deciding which scores are significantly high. Surely it will be possible in a legislature or assembly that operates by achieving high consensus and avoiding roll calls on divisive issues, to obtain the result that every member of the legislature is in significant agreement with every other member of the legislature? Of course this is true, but it would be a major finding about the behavior of the legislature. We would not have discovered any cluster blocs within the legislature but we would have firmly established that such divisions as there were either were very weak or were not expressed in the roll calls. Therefore, the first examination of the scores should always be against the null hypothesis of random voting. Indeed we now have for any legislature of M members an Index of Agreement, I_α, that is statistically based.

$$I_\alpha = \frac{\text{Number of pairs of members in significant agreement}}{\text{Total possible number of pairs of members}}$$

$$= \frac{\text{Number of pairs in significant agreement}}{M(M-1)/2}$$

Similarly, for a legislature that has deep divisions, we have an Index of Significant Disagreement, I_β,

$$I_\beta = \frac{\text{Number of pairs in Significant Disagreement}}{M(M-1)/2}$$

The two give an overall Index of Cohesion, $I_\gamma = I_\alpha - I_\beta$. In theory it will be possible for I_γ to obtain a negative value by the number of pairs in disagreement being higher than the number of pairs in agreement, but this is not very likely to occur in practice. I_γ should not be reported without the corresponding values of I_α and I_β, and all

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[19] This is not always true as there are some discontinuities in the results due to percentaging on a relatively low base, e.g., at the .05 level, the cutoff for 110 roll calls is 59.1 per cent which is higher than that for 100 roll calls. More detailed results are available on request.

Table 2. Percentage Cutoff Points for Cluster-Bloc Analysis: Simple Indices of Identical Voting*

No. of Roll calls	Significance Levels for Single-Tailed Tests							
	Disagreement				Agreement			
	.001	.01	.025	.05	.05	.025	.01	.001
4	—	—	—	—	—	—	—	—
5	—	—	—	0.0	100.0	—	—	—
6	—	—	0.0	0.0	100.0	100.0	—	—
7	—	0.0	0.0	0.0	100.0	100.0	100.0	—
8	—	0.0	0.0	12.5	87.5	100.0	100.0	—
9	—	0.0	11.1	11.1	88.9	88.9	100.0	—
10	0.0	0.0	10.0	10.0	90.0	90.0	100.0	100.0
11	0.0	9.1	9.1	18.2	81.8	90.9	90.9	100.0
12	0.0	8.3	16.7	16.7	83.3	83.3	91.7	100.0
13	0.0	7.7	15.4	23.1	76.9	84.6	92.3	100.0
14	7.1	14.3	14.3	21.4	78.6	85.7	85.7	92.9
15	6.7	13.3	20.0	20.0	80.0	80.0	86.7	93.3
16	6.3	12.5	18.8	25.0	75.0	81.3	87.5	93.8
17	5.9	17.7	23.5	23.5	76.5	76.5	82.4	94.1
18	11.1	16.7	22.2	27.8	72.2	77.8	83.3	88.9
19	10.5	21.1	21.1	26.3	73.7	79.0	79.0	89.5
20	10.0	20.0	25.0	25.0	75.0	75.0	80.0	90.0
21	14.3	19.1	23.8	28.6	71.4	76.2	81.0	85.7
22	13.6	22.7	22.7	27.3	72.7	77.3	77.3	86.4
23	13.0	21.7	26.1	30.4	69.6	73.9	78.3	87.0
24	16.7	20.8	25.0	29.2	70.8	75.0	79.2	83.3
25	16.0	24.0	28.0	28.0	72.0	72.0	76.0	84.0
26	15.4	23.1	26.9	30.8	69.2	73.1	76.9	84.6
27	18.5	25.9	25.9	29.6	70.4	74.1	74.1	81.5
28	17.9	25.0	28.6	32.1	67.9	71.4	75.0	82.1
29	17.2	24.1	27.6	31.0	69.0	72.4	75.9	82.8
30	20.0	26.7	30.0	33.3	66.7	70.0	73.3	80.0
40	22.5	30.0	32.5	35.0	65.0	67.5	70.0	77.5
50	26.0	32.0	34.0	36.0	64.0	66.0	68.0	74.0
60	28.3	33.3	35.0	38.3	61.7	65.0	66.7	71.7
80	31.3	36.3	37.5	40.0	60.0	62.5	63.8	68.8
100	34.0	37.0	39.0	41.0	59.0	61.0	63.0	66.0
120	35.0	38.3	40.0	41.7	58.3	60.0	61.7	65.0
140	36.4	39.3	40.7	42.1	57.9	59.3	60.7	63.6
160	37.5	40.0	41.9	43.1	56.9	58.1	60.0	62.5
180	37.8	40.6	42.2	43.3	56.7	57.8	59.4	62.2
200	38.5	41.5	42.5	43.5	56.5	57.5	58.5	61.5
250	40.0	42.4	43.6	44.4	55.6	56.4	57.6	60.0

* Probability of Agreement = 0.5; Probability of Disagreement = 0.5.

three should be given at the chosen level of significance. This is because both I_α and I_β may become higher at a lower level of significance.

Just as these three indices may be used to examine the cohesion of the whole legislature they may also be used to examine specified subgroups. The following results, for example, were obtained in a study of the behavior of the African group in the United Nations General Assembly.[20]

When the internal cohesion of specified groups is under consideration it may also be of interest to know how strong is the relationship *between* the groups. For example, are the Africans closer to the NATO or to the Warsaw Pact powers? Since the first step in cluster-bloc analysis would be to calculate agreement scores for every state with every other state, we may examine the level of agreement between every member of NATO and every member of the

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[20] Peter Willetts, "The Behavior of the African Group in the UN General Assembly" (unpublished M.Sc. Dissertation, University of Strathclyde, Scotland).

African group. For example, if the agreement score between France and Madagascar were found to be significantly high, this would constitute a "link" between the two groups. In general if the groups are of size M_1 and M_2 we may define the following indices to examine the relationship between the groups:

Inter-Group Agreement

$$IG_\alpha = \frac{\text{No. of significant links between the groups}}{\text{Total possible no. of links between the groups}}$$

$$= \frac{\text{No. of significant links}}{M_1 \times M_2}$$

Inter-Group Disagreement

$$IG_\beta = \frac{\text{No. of significant disagreements between the 2 groups}}{M_1 \times M_2}$$

Inter-Group Adhesion

$$IG_\gamma = IG_\alpha - IG_\beta$$

We have already seen in Table 3 that the African Group was relatively disunited in the United Nations 15th Session, but in subsequent sessions it became more united until in the 20th Session I_α attained 100 per cent at the .001 significance level. The earlier lack of unity might be explained by the fact that during 1960 the African states formed two rival groups that met regularly outside the United Nations. They became known as the Casablanca and Brazzaville groups. The Inter-Group Indices may be used to examine whether or not the differences within the African group in the UN were an expression of the split between these two rival groups.[21]

The indices in Table 4 clearly show a split in the 15th Session and a rapid change in subsequent sessions; by the 20th Session, the two groups show no significant differences at all. The increase in cohesion of the African group as a whole was mainly due to the decrease in hostility between the two rival subgroups. Several explanations of such patterns of change are possible. In this case the increased adhesion is due not so much to *rapprochement* of the two groups as to the resolution or diminution of the four main disputes that originally led to the formation of the groups: Mauritania gained admission to the UN; Algeria gained its independence from France; the bitterness of the disputes in the Congo were somewhat eased; and Nkrumah had to accept failure for his own militant brand of Pan-Africanism.

One of the original reasons for suggesting that cluster-bloc analysis was a more useful methodology was the advantage it offered of permitting empirical determination of the voting groups. But by using the voting agreement scores and the above indices we can also examine *any* specified group that is of interest. The indices are statistically based and are simple to comprehend. They have the same meaning when groups of different sizes are compared. By contrast, if Hovet tells us that a group is divided 50 per cent of the time, we do not know whether there is just one deviant member (in which case I_α would remain quite high), or whether the group

Table 3. Cohesion of the African Group in the UN

Session	$p \leq .001$			$p \leq .05$		
	I_α	I_β	I_γ	I_α	I_β	I_γ
15th	56.0%	0%	56.0%	72.7%	1.0%	71.7%
16th	90.8%	0%	90.8%	100.0%	0%	100.0%
17th	77.1%	0%	77.1%	96.2%	0%	96.2%
18th	89.5%	0%	89.5%	99.6%	0%	99.6%
20th	100.0%	0%	100.0%	100.0%	0%	100.0%

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[21] Willetts, Chap. 4. Both Tables 3 and 4 are based on the use of Lijphart's Index of Agreement for UN votes, discussed later in this paper.

Table 4. Casablanca-Brazzaville Inter-Group Agreement

Session	$p \leq .001$			$p \leq .05$		
	IG_α	IG_β	IG_γ	IG_α	IG_β	IG_γ
15th	1.8%	0%	1.8%	12.7%	5.4%	7.3%
16th	45.0%	0%	45.0%	100.0%	0%	100.0%
17th	48.6%	0%	48.6%	100.0%	0%	100.0%
18th	73.6%	0%	73.6%	98.6%	0%	98.6%
20th	100.0%	0%	100.0%	100.0%	0%	100.0%

is regularly split into two different factions (in which case I_α would be low and I_β might be high).

Significant Relative Agreement

As suggested earlier, a group might achieve such a high consensus that every member was in significant agreement with every other member. Also we have seen that this extreme result did occur for the African group in the United Nations 20th Session. Knowing this does not mean that one cannot say anything about the structure of such a group. It simply means that the group is homogenous and united, as compared to any model of random voting.

In any one session of a legislature or assembly, however, the issues on which the members are called upon to vote do not present a random stimulus. One session may produce a very high consensus simply because no controversial issues arise or lead to roll calls during the period that it is meeting. In this situation we may take the total votes on any one roll call as an approximate measure of the stimulus provided by the roll call. Thus if the U.S. Senate votes 63–37 in favour of a bill, we may say for any individual Senator the probability of voting "Aye" was 0.63 and the probability of voting "Nay" was 0.37. The resulting probability that any two Senators will agree is

$$p_f = 0.63^2 + 0.37^2 = 0.534$$

and the probability that they will disagree is

$$p_d = 2 \times 0.63 \times 0.37 = 0.466$$

If such a calculation is made for each roll call during the session, the average probability of agreement for the session can be obtained. Then, by substituting these new values of p_f and p_d in the equations given earlier, a different sampling distribution is generated. Since random voting, with an average 50–50 split on all roll calls, represents the maximum possible division within a legislature, any values of p_f calculated empirically from the voting marginal totals are likely to be higher than 0.5. The sampling distribution could then no longer be normal but would be skewed toward 100 per cent. As the argument has not rested on the assumption of normality, the skew does not mean we cannot obtain significant cutoff points. In fact, the exact probability for every point on the sampling distribution can be calculated. Therefore we still obtain exact significance levels from the tails of the distribution. To distinguish these cutoff points from the ones obtained by comparison to random voting we may term them the levels of significant *relative* agreement. They may only be used in a particular assembly to examine alignments on a given set of roll calls.[22]

Judicial Decisions

Very often in the study of judicial decisions, a large number of cases are summarized by using cluster-bloc analysis. Indeed, in many ways this is an ideal use of the methodology, because it is possible to gain a better understanding of all the interrelationships in a small body such as a court. Courts may operate, however, with a high degree of unanimity and therefore, against the null hypothesis of random voting, the court may appear to be completely united, with every judge in significant agreement with every other judge. One response to such a situation is to regard it as essential rather than optional to study significant *relative* agreement. An alternative response is to eliminate from the study the unanimous decisions and perhaps those with only a single dissenter. Schubert has analyzed split decisions and has even constructed matrices of agreement scores solely for marginal decisions of the U.S. Supreme Court.[23]

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[22] Details of the computer program for those that need to calculate cutoff points for significant relative agreement are available on request. Tables for values of $p_f = 0.6$, 0.7, 0.8, and 0.9 have already been computed and are also available.

[23] Glendon A. Schubert, *Quantitative Analysis of Judicial Behavior* (Glencoe, Illinois: Free Press, 1959). Unanimous decisions constituted about 30 per cent of all cases (p. 81). For marginal decision matrices see p. 117 and pp. 149–53.

Russell in analyzing the Canadian Supreme Court had to omit as many as 70 per cent of the decisions because they were unanimous.[24] Such omissions are perfectly acceptable so long as the focus of analysis is the nature of the divisions within a court. But if we wish to study whether division is increasing or decreasing over time, then unanimous decisions cannot be ignored.

Because of the special nature of a dissenting legal opinion, several authors have taken to studying agreement in assent and agreement in dissent separately from the overall pattern of agreement.[25] When this is done, we are reverting to Beyle's preoccupation with agreement on one side of an issue. The null hypothesis of random voting then gives a probability of agreement of 0.25 (derived from either Yes/Yes pairs or No/No pairs on their own, according to whether agreement in assent or agreement in dissent is taken). This probability value makes the cutoff points much lower (table available on request). Even though the cutoff points are low, joint dissent is likely to be so infrequent that no bloc structure is revealed amongst the agreement-in-dissent scores. This is not so likely to occur when the agreement-in-assent scores are examined.[26] If we use the *relative* probability of agreement in dissent, it is likely to be much lower even than 0.25, and the *relative* probability of agreement in assent will be much higher.

It frequently happens in a court that there is only one voice in dissent. This is in contrast to a legislative body, where one person will probably avoid being seen in such isolation. In legislatures, the dissenter may either be absent for the vote or a roll call may be avoided and thought unnecessary by the majority. In the courts, when there is only one dissenter, he obviously cannot be in agreement with anyone else. This means that such decisions cannot be taken into account in calculating the average probability of agreement in dissent, though they *can* be used in calculating the average probability of agreement in assent. Given this problem, it is not possible to approximate to the average probability of dissent by simply taking (total number of dissents)/(total possibilities for dissent). If there is a large number of decisions with single dissenters, the average probability of agreement in dissent will be *very* low and it may not be possible to find significant pairs.

United Nations Voting

When we wish to study voting in the United Nations, the problem is complicated by abstentions. The General Assembly accepts this as a recognised form of voting that is entered into the Official Records along with the details of those voting Yes or No. It is not equivalent to a state being absent and is used to express limited opposition to a proposal. Both Rieselbach and Meyers took this into account by including joint abstentions in their indices of identical voting for pairs of states.[27] On the other hand Lijphart went further and suggested that allowance should also be made for the fact that Yes-Abstain or No-Abstain pairs are in "partial agreement," he defined an Index of Agreement:

$$I_A = \frac{(f + \frac{1}{2}g)}{t} \times 100,$$

where

f = the number of identical roll calls, for the two states,

g = the number in which the states showed partial agreement,

t = the number that both states participated in.[28]

The assumption of random voting in the UN would require: $P(\text{Yes}) = P(\text{No}) = P(\text{Abstain}) = 1/3$. There are then nine possible outcomes, when two states vote, and each has a probability of $1/3 \times 1/3 = 1/9$.

	A's vote	*B's vote*	
1)	Yes	Yes	—Agreement
2)	No	No	
3)	Abstain	Abstain	
4)	Yes	Abstain	—Partial Agreement
5)	Abstain	Yes	
6)	No	Abstain	
7)	Abstain	No	
8)	Yes	No	—Disagreement
9)	No	Yes	

Using the same symbols, p_f = probability of agreement, and p_d = probability of disagreement, with the new symbol p_g = probability of partial agreement, we have:

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[24] Peter Russell, *The Supreme Court of Canada as a Bilingual and Bicultural Institution* (Ottawa: Queen's Printer, 1969), 127.

[25] C. Herman Pritchett, *Civil Liberties and the Vinson Court* (Chicago: University of Chicago Press, 1954); C. Herman Pritchett, *The Roosevelt Court: A Study in Judicial Politics and Values* (New York: Macmillan, 1948); see also Schubert, *Quantitative Analysis. . . .* ; and Russell *The Supreme Court of Canada. . . .*

[26] For example, on p. 132 of Russell the two joint dissent matrices only yield one pair of judges in significant agreement, whereas on p. 133 the joint assent matrices clearly show a bloc structure.

[27] Rieselbach, "Quantitative Techniques . . . ," pp. 292–293. Benjamin D. Meyers, "African Voting in the United Nations General Assembly," *Journal of Modern African Studies*, 4 (1966), 213–227.

[28] Lijphart, "Analysis of Bloc Voting . . ." p. 910

$$p_f = 1/3 \quad p_g = 4/9 \quad p_d = 2/9 \quad \text{and}$$

$$p_f + p_g + p_d = 1.0$$

For those such as Reiselbach and Meyers, who use simple indices of identical voting to study the UN, the argument given above still applies. The same table may be used as for any other legislature, such as the Congress, which uses simple Yes-No voting. The only difference is that random voting would not give $p_f = 0.5$, but $p_f = 0.3333$ and as partial agreement is ignored $p_d = 0.6666$. The levels of significant *relative* agreement are found as before by calculating the average empirical value of p_f.[29] For N roll calls:

$$p_f = \frac{\sum [P(\text{Yes})^2 + P(\text{No})^2 + P(\text{Abstain})^2]}{N}$$

For those who wish to use Lijphart's Index of Agreement, the situation is a little more complicated. Not only does the use of partial agreements give more discrete values that the index may assume, but it also means that the same value of the index may be produced by different voting patterns. For example when votes are taken on 6 roll calls, there are not seven but thirteen possible scores, which may be formed from twenty-eight possible voting patterns, as given in Table 5.

The probability equations are also more complicated, as multinominal distributions are generated for each agreement score, e.g.,

$$\begin{aligned} P(I_A = 50.0\%) &= C^6_3 \cdot p_f{}^3 \cdot C^3_3 \cdot p_g{}^0 \cdot p_d{}^3 \\ &\quad + C^6_2 \cdot p_f{}^2 \cdot C^4_2 \cdot p_g{}^2 \cdot p_d{}^2 \\ &\quad + C^6_1 \cdot p_f{}^1 \cdot C^5_4 \cdot p_g{}^4 \cdot p_d{}^1 \\ &\quad + C^6_0 \cdot p_f{}^0 \cdot C^6_6 \cdot p_g{}^6 \cdot p_d{}^0 \\ &= 20\left(\frac{1}{3}\right)^3\left(\frac{2}{9}\right)^3 \\ &\quad + 90\left(\frac{1}{3}\right)^2\left(\frac{4}{9}\right)^2\left(\frac{2}{9}\right)^2 \\ &\quad + 30\cdot\frac{1}{3}\left(\frac{4}{9}\right)^4\cdot\frac{2}{9} \\ &\quad + \left(\frac{4}{9}\right)^6 \\ &= 0.2001 \end{aligned}$$

Table 5. All possible outcomes when two states vote in the U.N. on six roll calls

	$f+\frac{1}{2}g$	I_A (%)	Structure of the voting pattern: Agreement	Partial Agreement	Disagreement
1)	6.0	100.0	6	0	0
2)	5.5	91.7	5	1	0
3)	5.0	83.3	5	0	1
4)	5.0	83.3	4	2	0
5)	4.5	75.0	4	1	1
6)	4.5	75.0	3	3	0
7)	4.0	66.7	4	0	2
8)	4.0	66.7	3	2	1
9)	4.0	66.7	2	4	0
10)	3.5	58.3	3	1	2
11)	3.5	58.3	2	3	1
12)	3.5	58.3	1	5	0
13)	3.0	50.0	3	0	3
14)	3.0	50.0	2	2	2
15)	3.0	50.0	1	4	1
16)	3.0	50.0	0	6	0
17)	2.5	41.7	2	1	3
18)	2.5	41.7	1	3	2
19)	2.5	41.7	0	5	1
20)	2.0	33.3	2	0	4
21)	2.0	33.3	1	2	3
22)	2.0	33.3	0	4	2
23)	1.5	25.0	1	1	4
24)	1.5	25.0	0	3	3
25)	1.0	16.7	1	0	5
26)	1.0	16.7	0	2	4
27)	0.5	8.3	0	1	5
28)	0.0	0	0	0	6

Even on random voting the distributions are not normal but slightly skewed toward 100 per cent. Although the procedure is more complicated, the probability of each point on the distribution can still be calculated, and the cutoff points for significant agreement or for significant disagreement are found at the tails of the distribution. The results for varying numbers of roll calls, with the assumption of random voting, are given in Table 6.

When Lijphart studied 44 roll calls on colonial issues in the 11th and 12th Sessions of the

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[29] Tables with p_f = 0.333, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9 or details of computer program available on request.

Table 6. Percentage Cutoff points for Cluster-Bloc Analysis. Lijphart Index of Agreement for UN voting*

Number of Roll Calls	Significance Levels for Single-Tailed Tests							
	Disagreement				Agreement			
	.001	.01	.025	.05	.05	.025	.01	.001
2	—	—	—	0.0	—	—	—	—
3	—	—	0.0	0.0	100.0	—	—	—
4	—	0.0	12.5	12.5	100.0	100.0	—	—
5	0.0	10.0	10.0	20.0	90.0	100.0	100.0	—
6	0.0	16.7	16.7	25.0	91.7	91.7	100.0	—
7	7.1	14.3	21.4	28.6	85.7	85.7	92.9	100.0
8	12.5	18.8	25.0	25.0	81.3	87.5	93.8	100.0
9	11.1	22.2	27.8	27.8	83.3	83.3	88.9	94.4
10	15.0	25.0	30.0	30.0	80.0	85.0	85.0	95.0
11	18.2	27.3	27.3	31.8	77.3	81.8	86.4	90.9
12	20.8	25.0	29.2	33.3	75.0	79.2	83.3	91.7
13	19.2	26.9	30.8	34.6	76.9	80.8	80.8	88.5
14	21.4	28.6	32.1	35.7	75.0	78.6	82.1	89.3
15	23.3	30.0	33.3	36.7	73.3	76.7	80.0	86.7
16	25.0	31.3	34.4	37.5	75.0	75.0	78.1	84.4
17	26.5	32.4	35.3	38.2	73.5	76.5	79.4	85.3
18	25.0	33.3	36.1	38.9	72.2	75.0	77.8	83.3
19	26.3	34.2	36.8	39.5	71.1	73.7	76.3	84.2
20	27.5	35.0	37.5	40.0	72.5	75.0	77.5	82.5
21	28.6	33.3	38.1	40.5	71.4	73.8	76.2	81.0
22	30.0	34.1	38.6	40.9	70.5	72.7	75.0	81.8
23	30.4	34.8	39.1	41.3	69.6	71.7	76.1	80.4
24	31.3	35.4	39.6	41.7	70.8	72.9	75.0	79.2
25	30.0	36.0	40.0	42.0	70.0	72.0	74.0	80.0
26	30.8	36.5	38.5	42.3	69.2	71.2	73.1	78.9
27	31.5	37.0	38.9	42.6	68.5	70.4	74.1	77.8
28	32.1	37.5	39.3	42.9	67.9	71.4	73.2	78.6
29	32.8	37.9	39.7	43.1	70.0	70.7	72.4	77.6
30	33.3	38.3	40.0	43.3	68.3	70.0	73.3	76.7
40	36.3	41.3	42.5	45.0	66.3	67.5	70.0	75.0
50	38.0	42.0	44.0	46.0	65.0	67.0	69.0	72.0
60	40.0	43.3	45.0	46.7	64.2	65.8	67.5	70.8
80	41.9	45.0	46.9	48.1	63.1	64.4	65.6	68.8
100	43.5	46.5	48.0	49.0	62.0	63.0	64.5	67.5

* Probability of Agreement = 0.333, Probability of Partial Agreement = 0.444, Probability of Disagreement = 0.222.

General Assembly, he used cutoff points of 95.5 per cent and 87.5 per cent to produce two diagrams of the cluster blocs. At the 95.5 per cent level, he found one bloc consisting of the Communist States, three small blocs of Western States, and an Arab bloc. Twelve other states had one or two isolated links but did not form a bloc. At the 87.5 per cent level, the Communist and Arab blocs merged and were highly intermeshed with the other Afro-Asians; the Western States and some of the Latin Americans also formed another large bloc, and the remainder of the Latin Americans had some links with the Afro-Asians. By using these high cutoff points Lijphart did find the core of the bloc structure but he also overemphasised the divisions within the Assembly. At the $p < .001$ level of significance, which is a high level of significance to use, it is necessary to go down to a cutoff point of 73.0 per cent for 44 roll calls. This would be almost certain to reveal a continuous structure of overlapping blocs, with the Latin Americans acting as a crucial group in the middle, having links with both sides. Thus the choice of cutoff points is not without substantive significance. By taking them too high one may overemphasize the splits in the Assembly.

Yet again it is possible to use cutoff points to indicate significant *relative* agreement for

United Nations voting. But as there are three variables, p_f, p_g, and p_d to take into account, there will be a greater variability in the reference tables.[30] The values of the relative probabilities of agreement, partial agreement, and disagreement may be calculated as follows:

$$p_f = \frac{\sum [P(\text{Yes})^2 + P(\text{No})^2 + P(\text{Abstain})^2]}{N}$$

$$p_g = \frac{\sum 2[P(\text{Yes}) + P(\text{No})] \cdot P(\text{Abstain})}{N}$$

$$p_d = \frac{\sum [2P(\text{Yes}) \cdot P(\text{No})]}{N} = 1 - p_f - p_g$$

It is also possible to use the Indices I_α, I_β, and I_γ to examine the cohesion of groups in the UN and the Indices IG_α, IG_β, and IG_γ to examine the adhesion between groups in the UN. Indeed the earlier examples of the use of these indices were taken from data on voting in the General Assembly.

The Investigation of Issue Dimensions

In factor analysis the votes on various roll calls may be regarded as observations about the individual legislators (or states) and the factors generated represent blocs that vote in a similar way. Alternatively the votes by various assembly members may be regarded as observations on the nature of the roll calls. Then the factors represent groups of roll calls that reflect a common issue dimension. Nobody has yet attempted such an approach with cluster-bloc analysis, but it can in fact be used in the two comparable ways. The technique need not be limited to the investigation of the voting blocs among assembly members but can also be used to investigate whether there are any *issue sets* that evoke a common response.

To discover issue sets it is first necessary to construct agreement scores for every possible pair of the roll calls. If just identical voting is considered, then Issue Similarity,

$$I_S = \frac{\text{No. of Assembly Members casting Identical Votes}}{\text{Total No. of members voting on both roll calls}} \cdot 100$$

Or if it is decided to allow for abstentions in United Nations voting, then Issue Similarity,

$$I_S = \left(\frac{i + \frac{1}{2}h}{s}\right) \cdot 100,$$

where

i = No. of states that cast identical votes on the two roll calls;

h = No. of states partially agreed on similarity of the roll calls, i.e., voting Abstain on one and Yes or No on the other;

s = Total No. of states that vote on both roll calls.

Once the Issue Similarity scores have been calculated we wish to know which scores are high enough to give pairs of significantly similar roll calls. Two new symbols may be used.

p_i = probability of identical voting on the roll calls;

p_o = probability of opposite voting on the roll calls.

With a simple index which ignores abstentions, a legislator may vote in one of only four different ways on two roll calls (1) Yes/Yes, (2) No/No, (3) Yes/No and (4) No/Yes. With random voting each has a probability of one in four, and thus $p_i = 0.5$ and $p_o = 0.5$. The equations for Issue Similarity scores will be the same as those for voting agreement between legislators. Therefore Table 2 gives the same cutoff points for significantly similar roll calls.

If a group of roll calls form a set in which every roll call is significantly similar to every other roll call in the set, then we may take it that the roll calls all represent different aspects of a common underlying issue dimension. But according to the logic it is also possible to find pairs of roll calls that are significantly dissimilar. In this case, we would also conclude that the two roll calls represent the *same* dimension. Very often in roll-call voting there is a switch in the way those with the same attitudes must vote in order to express that attitude, e.g., a conservative American Senator who considers that India is becoming socialist might vote *in favor* of an amendment to the Foreign Aid Bill to deny all aid to India, and then when the amendment fails would vote *against* the Bill as a whole. Such a pair of roll calls would then have a high dissimilarity and should be part of the same issue set. There must of course be a congruent pattern to any sets made up from roll calls that are significantly similar to some roll calls while being significantly dissimilar to others in the set. In general if A and B are similar while A and C are dissimilar, then B and C must be dissimilar as well, in order to make A, B, and C form a set.

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[30] More detailed computations against the null hypothesis of random voting or for other specified values of p_f, p_g, and p_d will be undertaken on request.

If Issue Similarity scores are calculated for United Nations roll calls including Abstain/Abstain pairs but ignoring partial similarity pairs, then $p_i = 0.3333$ and $p_o = 0.6667$. If partial similarity is included in the Index, then the probability of identical voting $p_i = 0.3333$, of opposite responses $p_o = 0.2222$, and of partially similar responses $p_h = 0.4444$, and the cutoff points are the same as those in Table 6.

Just as we may study the significant relative agreement of legislators in relation to a particular set of roll calls, it is also possible to study the *significant relative similarity* among issues. In other words, identical motions or bills formulated in an identical way may produce different results in different legislatures or even in the same legislature at a different time. Therefore, by analogous reasoning to that given earlier, values of p_i, p_o and, if necessary, p_h may be determined empirically, and the appropriate significance tables may be calculated.

Conclusions

In order to avoid being mesmerized by figures, one must reiterate the limitations that are inherent in all methods of legislative roll-call analysis. In the first place, the data consist solely of the recorded votes. While each legislator is thus making a conscious and deliberate public commitment, his recorded vote is still only a partial manifestation of the legislator's attitude. It has already been well established that in elections the individual citizen voter casts his vote on the basis of long established party loyalties or on just the broad outlines of the issues. Similarly, legislators usually vote on the broad principles of a bill, the technicalities or legal points usually being disposed of in committees. Thus roll calls do provide a coherent set of data that indicate alignments on the main issues. Judicial decision making should perhaps be treated with more caution. The strictest interpretation of the role of a judge asserts that neither political nor moral attitudes should have any effect on judicial decisions. Although in America, at least, it is well established that the attitudes of judges are reflected in their decisions, they are certainly not the only determinant factors.

Second, while the establishment of statistical significance is of great value it is not an end in itself. It does not *eliminate* the risk of error but just gives us a *known* risk of error: for example, in the UN 20th Session there were 117 states, which gives 6786 possible pairs of states. At the $p \leq .001$ level of significance we would expect to be wrong 7 out of 6786 times in assigning agreement or lack of agreement to a pair of States and at the $p \leq 0.05$ level we would expect to be wrong with as many as 34 pairs. It may be stating the obvious, but it is necessary to stress that any voting pattern discovered on the basis of statistical significance is not necessarily one that is *substantively* significant.

Thirdly, cluster-bloc analysis is a purely descriptive technique. It does not in itself provide any explanation of political processes. Very often we may wish to use it for simple exploration, asking such questions as, What blocs exist? How stable are they over time? The results may then suggest theories or may be used to support theories, but are not a substitute for theory. Indeed with the average legislature so much data will be generated that the researcher will be lost unless he knows beforehand exactly what he is looking for. There will probably be several hundred issue similarity scores and several thousand legislator agreement scores. Even very simple propositions will help in handling this amount of data.

Cluster-bloc analysis has long been established as one of the main methods available for legislative roll-call analysis. It can now become *the* main method, especially since the widespread use of computers in the social sciences means that the vast amount of labor formerly required should no longer be a hindrance to its use. Now that the method can be shown to have a sound statistical base, there is no longer any reason for the researcher to be forced to take arbitrary decisions that may greatly affect the results. The methodology is much strengthened. The use of summary indices that give limited information about predetermined groups is no longer necessary. The data itself can be used to discover empirically what are the voting blocs, but they can also be used to study any specified group. Factor analysis need no longer be considered a superior methodology, since it requires some substantial manipulation of the data and dubious assumptions about the level of measurement and normality.[31] Cluster-bloc analysis does not require this, and it allows the researcher to remain closely in touch with his data. Useful indices of cohesion within a group and adhesion between groups may be derived from the agreement scores.

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[31] See for example Alker and Russett, "World Politics . . . ," p. 30, which uses a complicated process of "standardization" of UN voting data. See also the references in note 7, for other authors that share my unease with factor analysis.

Special relationships within the assembly may be examined.[32] Finally the methodology can also be adapted to find issue sets that indicate underlying issue dimensions. If the cluster blocs are then examined on each of the issue sets, we have a very powerful method of analysis.

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[32] For example, by taking agreement scores between the U.S. and all small states on East/West issues in the UN and using the cutoff points given in Table 6, it was possible to determine for each state whether it was significantly aligned or not: H. Hveem and P. Willetts, "The Practice of Non-Alignment: on the Present and the Future of an International Movement," Department Short Study, Department of Political Science and Public Administration, Makerere University, Kampala, Uganda. With factor analysis a Western bloc would be found, but there is no way of determining whether the loading on this factor for a particular state is statistically significant or not.

COMMUNICATIONS

To the Editor:

In a recent article on "Party Effort and Its Impact on the Vote" (*American Political Science Review*, 65 [June, 1971], 439–450), William Crotty claims to show that "party effort" has a measurable impact on election outcomes. Unfortunately, the evidence Crotty displays is insufficient either to confirm or to disconfirm his hypothesis. With North Carolina counties as observations, Crotty regresses the vote divisions for five offices on five demographic variables, "party competition," and eighteen measures of "party effort" (nine per party). The main problem is that Crotty chose to report only that each entry of new variables into the equations generally increases the explained variance by a margin he regards as substantial, without revealing the strengths or even the signs of the relationships between the entered variables and the vote.

For example, Crotty shows that the entry of "party competition" into the equations pushes the multiple R's 3 to 6 points beyond what they were with only the demographic measures as independent variables. If this relationship between party competition and the vote is indeed meaningful, as Crotty suggests, it would be helpful to learn whether the sign of the relationship implies that party competition causes Democratic voting or implies that it causes Republican voting. Whichever the case, the results are at variance with current notions of the range of the explanatory power of "party competition." By extension, it is desirable to learn more about what happened when the eighteen "party effort" indicators were plugged into the equations—beyond the statement that the multiple R's increased from 2 to 12 points. Even if some of the 18 partial correlations (per equation) between effort and the vote are nonsensical, positive relationships between Republican effort and Democratic voting or between Democratic effort and Republican voting, they would—as any independent variable would—add somewhat to the explained variance.

Finally, a particularly acute problem arises with Crotty's interpretation of his finding that for competitive counties alone, the multiple R increases by 17 to 38 points when the "party effort" measures are cranked in. Crotty's conclusion is that "party effort during campaigns in competitive areas has a pronounced effect on the outcomes of the races" (pp. 446/447). But no such inference can be made from the data, because with so many variables, restriction of the analysis to competitive counties allows only 0 degrees of freedom—a level at which all results are meaningless. As indication, the footnote to Crotty's Table 5 tells that even the mammoth .98 multiple R's so derived fail to achieve "statistical significance." Indeed, with no degrees of freedom (a completely determined system), any deviation of the R from a perfect 1.00 must of necessity be solely due to computer rounding error.

In short, Crotty has applied some interesting data to an interesting problem. But the way he chose to present the findings prevents us from learning whether or not his hypothesis is correct.

Robert S. Erikson

Florida State University

To the Editor:

Mr. Erikson exaggerates. The research intent of the article is clear, and the execution follows closely the lines of development indicated at the beginning of the piece.

A number of interesting and related lines of inquiry could have been followed. Several of the most relevant of these are developed in the Appendix—including a reference to the exploration of the type of problem he finds most intriguing.

My major concern in the article, as I stated, was to explain total results rather than to trace the specific linkages. The increases in the explained variance related to the measures of party effort introduced are, in most cases, so substantial that it was hardly conceivable to treat them as the results of random variables. They also are in accord with a substantially grounded literature as well as the theoretical guidelines that introduced the analysis.

Although the major research interest was the variance accountability rather than the direction of the contributions, Table A.2 was included to accommodate the differential impacts of the independent variables.

A close inspection of this table, the citations in the notes which refer to analyses of the problem, and the Appendix more generally should provide the basic answers to the questions raised.

William Crotty

Northwestern University

TO THE EDITOR:

Re: Gerald M. Kramer "Short Term Fluctuations in U.S. Voting Behavior 1896–1964" *American Political Science Review*, 65 (March, 1971), pp. 131–143.

Gerald Kramer develops a most interesting study in which he seeks to explain the federal election outcomes by economic variables such as the growth of income, inflation, and unemployment. He specifically refers to his model as being in the tradition of Anthony Downs. This is perfectly correct with respect to voters who are assumed to be rational, but due to costs of information and decision act as "satisficers."

The model is, however, at odds with Downs because of the neglect of the *interdependence* between the government and voters. According to Downs:

> "Since governments plan their actions to please voters and voters decide how to vote on the basis of government actions, a circular relation of mutual interdependence underlies the functioning of government in a democracy" (p. 74 of *An Economic Theory of Democracy*, N.Y.: Harper, 1957).

This interaction is not only at the heart of the Downsian model, but an essential part of most political theories.

Thus there are two relationships to be considered:

(1) voters' reactions to economic conditions, and

(2) government's influence upon economic conditions in reaction to anticipated election outcome.

The fact that Kramer's model is not, as alleged, "Downsian" is of little importance, except insofar as it might lead to misunderstandings. It is important, however, that the results and conclusions reached may be doubtful if the interdependence mentioned is taken into account. The problem which arises is the classical simultaneous equation estimation problem of econometrics. There is not only the equation of the form

$$y(t) = \alpha_1 X_1(t) + \cdots + \alpha_n X_n(t) + u(t),$$

where

$y(t)$ = party A's share in the vote,

$X_i(t)$ = economic variable ($i = 1, 2, \cdots, n$),

$u(t)$ = random error of the classical regression model,

as estimated by Kramer, but also an equation describing the reaction of the government upon the vote share, e.g.:

$$X_i(t) = \beta_i y(t) + \epsilon(t),$$

where

$\epsilon(t)$ = random error

Combining the first and second equations, the expected value,

$$E\{X_i(t)u(t)\} = E\{\cdots + \beta_i u(t)u(t)\}$$
$$= \cdots + \beta_i \sigma_u^2,$$

will be zero only under extremely restrictive assumptions about u and E.

The formula shows that when there is interdependence, the economic variables $X_i(t)$ are no longer independent of the error term $u(t)$. It is well known (see e.g., J. Johnston, *Econometric Methods*, N.Y.: McGraw Hill, 1963, p. 141) that in that case the ordinary least-square estimation procedure used by Kramer for the first equation generally leads to biased and inconsistent results.

Kramer should have paid attention to the interdependence problem; the conclusiveness of his results must be left open. His model should be extended to take into account the reaction of governments to the expected election outcomes. To do this, a theory of government behavior must be used, a possible basis of which is Downs's vote maximizing assumption. This means that empirical political science must make the step from the estimation of single equations to whole models composed of several simultaneous equations, i.e., to move towards what may be called "*politometrics*."

HERMANN GARBERS and
BRUNO S. FREY

University of Konstanz

EDITORIAL COMMENT

Present and Absent. There is almost certainly a relationship between the general subject matter of articles that get printed in the *Review* at one point in time, and the subject matter of the next batch of articles that are submitted for publication. Our casual observation is that scholars who have articles up their sleeves are superattentive to what seems to be getting through the editorial maze, and may attempt to infer the editor's preferences from what ends up in print. If this impedes the *Review* from having a crack at good articles which are nevertheless unlike those we have published recently, it is our loss, and consequently we want to reaffirm that we are in business to serve the scholarly needs of every branch of our discipline.

A glance at articles appearing in the last full volume of the *Review* (Vol. 65, 1971) does reveal some meaningful clusters and some notable absences. There were 46 articles printed in Volume 65. Thirty-five of them included at least one table or figure. This attests to a preoccupation in our discipline with the manipulation of large numbers of examples. It suggests a growing collective capacity to deal with frequently occuring phenomena, including such things as the opinions of mass publics, votes, and responses to large-scale surveys. An important characteristic of frequently occurring phenomena is that properly arrayed and interpreted they can be used to assign known degrees of confidence to propositions offered as explanations for their occurrence or distribution. The fact that so much of the *Review* has recently been given over to this sort of material indicates that a sizable commitment is being made in the discipline to the expansion of what we can be reasonably sure we *know*.

Nineteen articles dealt with some aspect of political psychology or mass voting behavior, value and attitude acquisition by citizens, or low- and middle-level political participation. There is no question that we are in the midst of significant intellectual progress on this cluster of problems. Diverse perspectives are being brought to bear. Scholars are offering new data and new ways of stating questions for research. Although this is not the only main current in contemporary political science, it is without question one of the main currents. The *Review* has always had a major role in encouraging conversation and research in this area, and, apparently, this role is continuing.

Twenty articles were tied fairly closely to U.S. data; 12 were explicitly about activity in one or more foreign countries; and 14 were not classifiable on this basis. This is a somewhat less parochial showing than one might expect from an American journal.

We classified 13 articles as "formal theory." This meant that their texts contained sentences having Greek letters or algebraic expressions other than formulas for computing statistical tests. Given the spotty mathematical training of most practicing political scientists, and the consequent handicaps that they suffer in attempting to read mathematical material, this finding attests to the willingness of the *Review* to serve as a forum for small groups within the discipline and also, no doubt, to a desire to encourage the growth of new kinds of political science.

Because there is no concomitant wish to discourage more traditional kinds of political science, one deplores the weak representation in Volume 65 of contributions in public law (one American, one comparative), public administration and organization theory (one example), policy analysis (by a generous count, three examples), political philosophy (three articles), elite politics (two articles) and international politics (two articles).

How to explain these absences? We must not rule out the possibility—although it is likely no more than a possibility—that political scientists are simply becoming less interested in these subjects, and hence are writing less about them. No doubt the *Review* is seeing less than its share of articles in some subfields because these fields have first-rate journals of their own, like the *Public Administration Review* or *World Politics*, or *Ethics*, or any one of a flock of law journals. Or, conceivably, our standards are higher in old established areas of the discipline than in the new ones.

Some imbalance in the *Review* is probably inevitable over the short run and—in the short run—this is arguably even desirable. It would, however, be a matter of some concern if over a longer time span the *Review* found itself increasingly cut off from important intellectual currents in the discipline.

We have no sure way of telling whether or not the sample of articles that ultimately finds its way into the *Review* is in fact an accurate reflection of the best among the current intellectual preoccupations of political scientists. We can be reasonably confident that what we print is on the whole the best of what we see. But we should also guard against the possibility that we are not seeing all the different kinds of articles we should be seeing. It is not entirely clear how to go about doing this, except by pointing to the light in the window, and keeping the front door ajar.

On the Cover. One of the master strokes of the new administration of the *Review* has been to shift the bulk of complaints about the magazine from its contents to its cover. The cover of the *Review,* we feel, is something to sink one's teeth into, and although we are told it leaves a gluey aftertaste, it does provide much more nourishment than the ephemera not to say trivia that we have all complained about for so long. In view of widespread interest in the cover, we are pleased to report the following:

We carefully considered, but then rejected the idea of enclosing the *Review* in a plain brown wrapper. Our grounds were that to do so might constitute fraudulent advertising and might, under the rule enunciated by the Supreme Court in *Ginzburg v. United States,* 383 US 463 (1966) seriously discommode the Managing Editor and actually impair delivery of the *Review* to its by then panting subscribers.

From the solution of putting nothing on the cover we leaped to the opposite extreme and considered putting everything there. This, we feared, would make for a cluttered, unattractive, and well-nigh unreadable cover. It also would take from the interior of the magazine a certain element of surprise. How many prospective readers, we wondered, faced with all that exoskeletal prose, would decide not to probe beneath the surface?

Moderate counsel prevailed. It was decided to put just enough on the cover to assure the timid that indeed this publication is the *American Political Science Review,* and to help them differentiate one issue from the next by means of cleverly planted clues. For the reader with an encyclopedic mind, the first page always contains a full synoptic table of contents, and ever heftier appetites can be gratified by immediately succeeding pages of abstracts, and lists of correspondents and books reviewed. By such sneaky means we thought to draw the unwary into the journal proper. Further, we hoped to unclutter the cover sufficiently to permit a little imagination and attractiveness to enter into its design.

All this, alas, has come at a price. Some authors get only their names on the cover. Others get their names and in addition the title of their article. This is invidious. By what criteria are such marks of distinction meted out? Looking back over the past couple of issues, it seems apparent that we can pretty well rule out alphabetical order. Can it be that the editors of the *Review* play at dice in making up their table of contents? Do they follow, God forbid, some notion of individual preference? Are authors whose names appear above the title personal pals of the Managing Editor? Do they all have some secret connection with the University of Minnesota? Are they authors of the shortest contributions, or the longest? Are they the oldest contributors? Youngest? The mind reels at the possibilities. The editors of *PS* will, we know, be grateful to receive essays from Association members which in twenty-five words or less (or the equivalent in calculus) explain the handicapping system at work in designing covers for the *Review*. Entries must be dated April 1. Winning entries become eligible for inclusion in the special 1984 edition of *An End to Political Science*. Decisions of judges are final, and will be based on originality of expression and neatness. Employees of the Association, the *Review,* and holders of Yale PhD's are not eligible to compete.

Articles Accepted for Future Publication

Paul Abramson, Michigan State University, "Intergenerational Social Mobility and Partisan Choice"

Herbert B. Asher, Ohio State University, "The Learning of Legislative Norms"

David W. Brady, University of Houston, "A Research Note on the Impact of Inter-Party Competition on Congressional Voting in a Competitive Era"

Michael Brecher, McGill University, "Images, Process, and Feedback in Foreign Policy: Israel's Decisions on German Reparations"

Eric C. Browne, University of Georgia and Mark N. Franklin, University of Strathclyde, "The Perquisites of Government: Aspects of Coalition Payoffs in European Parliamentary Democracies"

Charles S. Bullock, III, University of Georgia, "Freshman Committee Assignments and Reelection in the U.S. House of Representatives" and "House Careerists: Changing Patterns of Longevity and Attrition"

Charles E. Butterworth, University of Maryland, "Averroës: Politics and Opinion"

Peter K. Eisinger, University of Wisconsin, Madison, "The Conditions of Protest Behavior in American Cities"

Robert S. Erikson, Florida State University, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections"

John Ferejohn, California Institute of Technology and Talbot Page, Resources for the Future, "A Note on 'Voting or a Price System in a Competitive Market Structure' "

Alexander L. George, Stanford University, "The Case for Multiple Advocacy in Making Foreign Policy"

Fred Kort, University of Connecticut, "Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science"

Carl H. Lande, University of Kansas, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics"

Wallace Mendelson, University of Texas, "From Warren to Burger: The Rise and Decline of Substantive Equal Protection"

Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"

Andre Modigliani, Harvard University, "Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy"

Sarah McCally Morehouse, Manhattanville College, "The State Political Party and the Policy-Making Process"

Donald G. Morrison and H. Michael Stevenson, York University, "Intergeneration and Instability: Patterns of African Political Development"

Edward N. Muller, State University of New York, Stony Brook, "A Test of a Partial Theory of Potential for Political Violence"

Walter Odajnyk, Columbia University, "The Political Ideas of C. G. Jung"

Benjamin I. Page, Dartmouth College and Richard A. Brody, Stanford University, "Policy Voting and the Electoral Process: The Vietnam War Issue"

Bruce W. Robeck, Texas A and M University, "Legislative Partisanship, Constituency and Malapportionment"

Thomas W. Robinson, Visiting Fellow, Council on Foreign Relations, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"

Howard Rosenthal, Carnegie-Mellon University, "Electoral Participation in the French Fifth Republic"

John Gerard Ruggie, Dotation Carnegie Pour La Paix Internationale, "Collective Goods and International Organization"

Lawrence A. Scaff, University of Arizona, "Max Weber's Politics and Political Education"

Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, University of North Carolina, Chapel Hill, "Political Socialization and Political Belief Systems: An Essay on the Theoretical Relevance of Some Current Research"

W. Phillips Shively, University of Minnesota, "Voting Stability and the Nature of Party Attachments in the Weimar Republic"

Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich and Craig E. Schopmeyer, Ohio State University, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models"

John L. Sullivan, Iowa State University and Robert E. O'Connor, Pennsylvania State University, "Electoral Choice and Popular Control of Public Policy: The Case of the 1966 House Elections"

John L. Sullivan, Iowa State University, "A Note on Redistributive Politics"

Herbert F. Weisberg, University of Michigan, "Scaling Models for Legislative Roll-Call Analysis"

From Gold to Garbage: A Bibliographical Essay on Politics and the Environment*

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> Is it surprising that a system seeking to turn everything into gold ends up turning everything into garbage?[1]

My grandfather, wise but unschooled, would have responded: "That ain't no question, it's an unanswer!" Well, the spate of books on the politics of environment, ecology, and pollution contains more unanswers than a Dr. Seuss encyclopedia. Why review them at all? There are practical reasons for doing so. First, text selections must be made and some of these books are better than others. Second, saving a colleague's time by sorting out the literature in a field probably serves a social purpose. Third, and most significantly, reviewing a set of books on a broad issue-area permits evaluation of (1) the base of knowledge for analyzing the substance of problems and the process of policy in the given issue-area; and (2) the application of political theories, generalizations and frameworks in analyzing emerging issues.

Though major environmental problems have been apparent since the first triumphs of the industrial revolution, environmental issues have come rather recently onto the federal domestic agenda. I have not surveyed all cases of dramatic issue emergence, but I suspect the pattern of policy response in the environmental case may be typical. Political rhetoric immediately focuses on the new issue. Participation in policy making is greatly expanded, without a corresponding increase in knowledge. Laws are passed, standards established, leaders selected, organizational changes made. Since typically, inadequate funds are appropriated, and staffing for new governmental functions is a slow process, these highly publicized symbolic actions buy time for an agency while it increases its knowledge and organizational capabilities to meet increments in authority.

Outside government, scholars and publicists also adjust priorities in an attempt to marshal knowledge on the issue. The process of information exchange is, on the whole, unsystematic, without being totally chaotic. Individual scholars tend to "do their own thing" unless grants are available. And even though public and private funding influences research priorities, there is so little monitoring of research output by government agencies and private foundations that neither proposals nor results are well coordinated or integrated. All of this supports Charles E. Lindblom's characterization of the policy process as "incremental."[2] Surely if we do not (or cannot) assess what we know, we obviously do not have the capability

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*Walt Anderson, ed., *Politics and Environment: A Reader in Ecological Crisis* (Pacific Palisades, California: Goodyear, 1970. Pp. 362. $5.50).
Lynton K. Caldwell, *Environment: A Challenge for Modern Society* (Garden City, New York: The Natural History Press, 1970. Pp. 292. $7.95.)
Congressional Quarterly, Inc., *Man's Control of the Environment* (Washington, D.C.: Congressional Quarterly, 1970. Pp. 91. $4.00.)
Council of Environmental Quality, *Environmental Quality: The Second Annual Report* (Washington, D.C.: Government Printing Office, 1971. Pp. 360. $2.00.)
J. H. Dales, *Pollution, Property & Prices* (Toronto: University of Toronto Press, 1968. Pp. 111. $2.50.)
J. Clarence Davies III, *The Politics of Pollution* (New York: Pegasus, 1970. Pp. 231. $6.00.)
Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal, *Environmental Control: Priorities, Policies, and the Law* (New York: Columbia University Press, 1971. Pp. 311. $9.00.)
Harold W. Helfrich, Jr., ed., *Agenda for Survival* (New Haven, Connecticut: Yale University Press, 1971. Pp. 234. $10.00.)
Huey D. Johnson, ed., *No Deposit-No Return: Man and His Environment* (Reading, Massachusetts: Addison-Wesley, 1970. Pp. 351. $2.95.)
Editors of *Ramparts, Eco-Catastrophe* (San Francisco: Canfield Press, 1970. Pp. 158. $2.95.)
Roger Revelle and Hans H. Landsberg, eds., *America's Changing Environment* (Boston: Houghton Mifflin, 1970. Pp. 314. $6.95.)
Leslie L. Roos, Jr., ed., *The Politics of Ecosuicide* (New York: Holt, Rinehart and Winston, 1971. Pp. 404. $4.95.)
Joseph L. Sax, *Defending the Environment: A Strategy for Citizen Action,* (New York: Knopf, 1971. Pp. 259. $6.95.)
Richard Saltonstall, Jr., *Your Environment and What You Can Do About It: A Citizen's Guide* (New York: Walker, 1970. Pp. 299. $6.95.)

This review was written during my stay at the Center for Advanced Study in the Behavioral Sciences. I gratefully acknowledge the Center's generous support.

[1] Editors of *Ramparts, Eco-Catastrophe* (San Francisco: Canfield Press, 1970), p. ix.

[2] See David Braybrooke and Charles Lindblom, *A Strategy of Decision: Policy Evaluation as a Social Process* (New York: Free Press, 1963).

of basing policy proposals entirely on knowledge.

Still other problems face political scientists in marshaling knowledge about the environment. With certain exceptions, concentrating on substantive issues has until very recently provided few professional rewards for political scientists. Indeed, the post–World War II phase in the discipline definitely de-emphasized policy study and recommendation. For research purposes, our subject matter has been organized more by political and social institution. This is not to say the resulting generalizations have no bearing on issues and how they are resolved. But we have tended not to apply them in that way. The recent demand for issue-relevant knowledge, then, requires changed emphasis in how we do our work and how we use our results. Given the obvious importance of political intelligence to environmental policy action, if political scientists do not produce this knowledge, others will try. This fact is amply illustrated in the set of books discussed here.

What may we reasonably expect of the literature on politics and the environment whether contributed by political scientists or others? The answer to this question is dictated by our knowledge needs. These are diverse but surely include description and analysis of the following:

1. The emergence of the issue.
2. Issue characteristics (e.g., problems, conflicts, organization and access of interests, public opinion) important for policy action.
3. Costs and effects of policy alternatives.
4. The institutional base for action.
5. Existing policy, its development and implementation.[3]

One would not expect each book to address all these topics. Rather the list directs our attention to knowledge needs, however general, that permit evaluation of the literature at hand.

Now to the books. They appear to group themselves as follows. First are those works that review the scope of the issue—presumably contributing to our understanding of the first and second categories above. The second group includes source materials useful for all categories. These books provide little or no analysis. They compile laws, review progress, seek to determine "where we are." A third set might be labeled "how to do it" books. Though not directly relevant to the list of knowledge needs, these works implicitly contribute to our understanding of the costs and effects of policy alternatives. Fourth, the economists have provided some literature of direct relevance to the first three categories above. And, finally, "the politics of . . ." books may be expected to contribute to our knowledge needs under the fourth and fifth categories. I have selected examples of each group for discussion, citing other works where appropriate.

The Scope of the Issue

These books are of several types—collections of conference papers, the "eco-scare" volumes, and general and scientific analyses of environmental issues. The conference or symposium is to scholars what hearings are to legislators or study commissions to presidents. Since attendance is limited, conference papers and remarks are published for wider distribution. Typically, these are of limited value. *No Deposit-No Return,* edited by Huey D. Johnson, and *Agenda for Survival,* edited by Harold W. Helfrich, Jr., are *not* exceptions to this rule.

The Johnson volume is a potpourri of statements by 65 of the participants at the 13th National Conference of the U. S. National Commission for UNESCO. An attempt was made by conference organizers to cover as many topics as possible. Thus, a little bit (contributions range from 1 to 11 pages) is said about a lot of things. The anthology begins with Arthur Godfrey, devoted environmentalist who finds time in his busy social-action schedule to sell Plymouths on television, entreating the conference:

> It's time to survive—to volunteer for survival.
> Good luck! Crud Ho!

The few useful statements get lost in such flotsam and jetsam. The book ends with several "action suggestions" developed during the meetings. These range from the obvious to the naive.

Agenda for Survival is the result of a symposium at the Yale School of Forestry. Public officials and other activists in conservation were invited to present formal lectures on various aspects of ecology. The result is a literate, if very general, call to action. The remarks by Edgar Berman on population limitation, Charles F. Wurster on DDT, and Harry M. Caudill on the compatibility of conservation and capitalism, are worth reading, but over-all the book lacks organization and clear purpose.

America's Changing Environment (edited by Roger Revelle and Hans H. Landsberg) stands out in this competition. Most of the papers were originally prepared for a conference in

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[3] Description and analysis of policy evaluation are also necessary but must follow experience in implementing recent policy and organizational breakthroughs.

1966 and were issue as the Fall, 1967 issue of *Daedalus.* Essays have been added to this thoughtful collection for publication in book form. The principal dimensions of the environmental issue are identified in this collection (though contributions are uneven in quality). Each contributor has enough space to present his argument, with supporting data; and those participating know enough to do more than "view with alarm." Political scientists will find informative the section on "Economics and Politics," though only Aaron Wildavsky's paper is strictly about political aspects of the problem.[4]

The "eco-scare" books vary from the "horror story, damn the Establishment" types to more objective efforts to outline the dimensions of the problem. *Eco-Catastrophe* stands on the far left of any such continuum. Issued by *Ramparts,* the book is slick journalism with a purpose:

> . . . the writers in this volume do not see the ecology crisis as merely one among a number of distinct social problems, which bear only a superficial relation to one another. On the contrary, the destruction of the living environment is seen to be an integral part of the general social crisis in America: the oppression of minority peoples, the genocidal war against the Vietnamese, and the accelerating repression of all militant forces for social progress and change (p. v).

The book contributes little to fulfilling those needs listed earlier. Although these articles contain some good reporting, unfortunately, one has no way of sifting the truth as it is from the truth as the authors want it to be.[5]

The most useful books in this first category are those seeking to provide objective descriptions and analyses of environmental issues. Understandably, most of these are written by biologists, chemists, meteorologists, ecologists.[6] Lynton Caldwell's book *Environment: A Challenge to Modern Society,* is the only one I know about in this genre written by a political scientist. Caldwell explores environmental quality as a social issue requiring political action. He carefully reviews the dimensions of the issue that complicate policy development and administration. He forcefully outlines the imperatives of integrative planning for environmental management. The book—combining philosophy, science, technology, politics, economics and administration—is a very useful introductory essay for courses on environmental control as well as for identifying major research needs.

Source Books

Given the changing nature of government action in environmental control, the scholar needs reliable source materials on problems, policy, personnel, and organization. The *Congressional Quarterly* has been providing this type of service on major issues for some years now. Unfortunately, *Man's Control of the Environment,* is not up to the standards set by other volumes in its public affairs series. The book reviews major environmental issues—air and water pollution, coastal pollution, solid waste disposal, land use, the power crisis, noise pollution, and other conservation problems. But coverage is maddeningly uneven. Legislation is fully reviewed in some areas, not in others. Little attention is paid to the agencies and congressional committees involved in decision making. And the coverage of major political infighting among elected leaders and lobbyists is spotty. One expects lumpiness in a document produced by "staff." The *Congressional Quarterly* has demonstrated in other issue-areas, however, that it can do better.

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[4] Other recent conference-symposium format books include Richard A. Cooley and Geoffrey Wandesforde-Smith, eds., *Congress and the Environment* (Seattle: University of Washington Press, 1970) and C. L. Wilson and W. H. Matthews, eds., *Man's Impact on the Global Environment: Assessment and Recommendations for Action* (Cambridge: MIT Press, 1970). Resources for the Future publications should also be mentioned in this category. See, for example, the very useful collection *Environmental Quality In a Growing Economy,* ed. Henry Jarrett (Baltimore: The Johns Hopkins Press, 1966).

[5] The current list of "eco-scare" books is too long to reproduce here. Some examples: Garrett De Bell, ed., *The Environmental Handbook* (New York: Ballantine Books, 1970); Jack Bregman and Sergei Lenormand, *The Pollution Paradox* (New York: Spartan Books, 1966); Editors of *The Progressive, The Crisis of Survival* (Glenview, N.J.: Scott, Foresman, 1970); Clifton Fadiman and Jean White, eds., *Ecocide . . . and Thoughts Toward Survival* (Palo Alto, Calif.: Freel, 1971), one of the more responsible collections —developed from conferences at the Center for the Study of Democratic Institutions; Lewis Herber, *Crisis in Our Cities* (Englewood Cliffs, N.J.: Prentice-Hall, 1965); and Ron M. Linton. *Terracide: America's Destruction of Her Living Environment* (Boston: Little, Brown, 1970). Of course, Rachel Carson's *Silent Spring* (Boston: Houghton Mifflin, 1962) remains a classic of the "eco-scare" volumes—and for good reason. See also, Frank Graham, Jr., *Since Silent Spring* (Greenwich, Connecticut: Fawcett, 1970). All of these are more balanced than *Eco-Catastrophe*—indeed, most include very useful material for understanding the scope of the issue.

[6] Again the list is very long, with items varying greatly in usefulness for the layman. In addition to the Resources for the Future publications, the nonscientist will find the report by the American Chemical Society's Subcommittee on Environmental Improvement, entitled *Cleaning Our Environment: The Chemical Basis for Action,* 1969, most helpful as an introduction to scientific aspects of environmental problems. A more popularly written item is that by biologist Barry Commoner, *The Closing Circle: Nature, Man and Technology* (New York: Knopf, 1971). See also William H. Whyte, *The Last Landscape* (Garden City, N.Y.: Doubleday, 1968).

Environmental Quality, the second annual report of the Council on Environmental Quality, is an excellent source book on environmental policy and organizational developments. The report summarizes all federal, state, and local activities in pollution control, waste management, and land use. Action by industry (including expenditures data on pollution control) and citizen groups is also reviewed. Special sections are devoted to the complex economic and legal aspects of pollution control and the special problems of the inner city. The organizational changes of 1969–1970 are explained, with all recent shifts charted. *Environmental Quality* is an excellent resource—highly recommended to the scholar both for research and classroom use.[7]

Another source book of value is *Environmental Control: Priorities, Policies, and the Law,* by Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal. This study was funded by the Sloan Foundation and the Council on Law-Related Studies. It consists of three papers—a superficial review of goals and priorities by Rathjens, a rather comprehensive survey of the intergovernmental aspects of environmental control by Grad, and a summary of federal enforcement and control techniques by Rosenthal. Of these, Grad's paper is the most valuable. Establishing a sharing pattern for regulatory authority within a federal system is a tediously complex matter. Though Grad barely scratches the surface of this topic, he is to be commended for this early effort to bring some order to the maze of federal, state, and local environmental statutes. This book is a resource for the scholar, bureaucrat, and legislator—*not* a classroom text.[8]

How To Do It Books

Recent public concern with the environment has spawned several books and pamphlets on how the citizen can help by collecting garbage, turning the lights out early, refusing nonreturnable bottles, and buying unleaded gasoline. Joseph L. Sax, *Defending the Environment: A Strategy for Citizen Action,* and Richard Saltonstall, Jr., *Your Environment and What You Can Do About It: A Citizen's Guide,* are somewhat more serious efforts and deserve comment here. Sax, a professor of law at the University of Michigan, argues that the administrative agency

> has supplanted the citizen as a participant to such an extent that its panoply of legal strictures actually forbid members of the public from participating even in the complacent process whereby the regulators and the regulated work out the destiny of our air, water, and land resources (p. xvii).

The remedy, Sax says, is to be found in the courts.

> . . . the court preeminently is a forum where the individual citizen or community group can obtain a hearing on equal terms with the highly organized and experienced interests that have learned so skillfully to manipulate legislative and administrative institutions. The court is attractive too because, free of the constraints which familiarity and close dealing tend to breed, it can bring fresh insights to problems of environmental management (p. xviii).

Professor Sax adds a new wrinkle to the case for participatory democracy. One does not work through elected officials, for they are all bought off. The citizen can only put his trust in the courts, which will in turn act as catalysts to action "in the public interest."

> . . . the role of courts is not to make public policy, but to help assure that public policy is made by the appropriate entity, rationally and in accord with

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[7] Congressional documents—staff reports and hearings—are also excellent source materials. Of particular note are the hearings before the Subcommittee on Conservation and Natural Resources of the House Committee on Government Operations—*The Environmental Decade,* 91st Cong., 2d sess., 1970. See also the hearings during the past ten years before the Subcommittee on Air and Water Pollution, Senate Committee on Public Works. Hearings before the Subcommittee on Public Health and Welfare, House Committee on Interstate and Foreign Commerce, *Air Pollution Control and Solid Wastes Recycling,* 91st Cong., 1st and 2d sess. 1969, 1970, thoroughly evaluate the Air Quality Act of 1967—thereby identifying many of the dimensions of administering environmental controls in a federal system. See also the extensive hearings on population growth conducted during the 1960s by the Senate Committee on Government Operations.

[8] Other source materials of value are Sidney Edelman, *The Law of Air Pollution Control* (Stamford, Conn.: Environmental Research & Applications, 1970); Stanley E. Degler and Sandra C. Bloom, *Federal Pollution Control Programs: Water, Air, and Solid Wastes* (Washington, D.C.: Bureau of National Affairs, 1969); Stanley E. Degler, *State Air Pollution Control Laws* (Washington, D.C.: Bureau of National Affairs, 1969); the many documents and reports produced by the Public Land Law Review Commission, Department of the Interior, the Advisory Commission on Intergovernmental Relations, the American Enterprise Institute, and the Subcommittee on Intergovernmental Relations, Senate Committee on Government Operations; and, of course, the continuing coverage and special reports in the *Congressional Quarterly Weekly Report* and *The National Journal.* Finally, several of the reports by Ralph Nader's Center for Study of Responsive Law fit this category. See, for example, John C. Esposito, *Vanishing Air* (New York: Grossman, 1970) and David Zwick and Marcy Benstock, *Water Wasteland* (New York: Grossman. 1971), which combine a great deal of information on the problems and critical analysis of government action.

the aspirations of the democratic process (p. 151).

One has a right to expect some evidence from the lawyer-author—both on the allegations and the expectations. For some reason, however, Sax does not preoccupy himself with presenting evidence. For example, he has not, as we might expect given the sweep of his allegations, analyzed the administration of environmental programs. Rather, he generalizes from a case of land use in Virginia. Admittedly the case is dramatic, and most would agree that it represents a fiasco in public decision making. Still, it is hard to take Sax seriously when he generalizes from this case to say (p. 56) that "the administrative process tends to produce not the voice of the people, but the voice of the bureaucrat. . . ." The words are so vague as to be virtually meaningless in serious discourse. And even if he bothered to define them, the story he tells about Hunting Creek, Virginia, hardly prepares us to draw the conclusions he wants.

His case for participatory democracy through the courts is further undermined by the effect of public opinion on lawmaking and policy implementation in 1969–1970. There is little or no evidence to show that the courts were the catalysts either for the surge in public interest in the environment or for the outpouring of legislation during 1969–71, at federal, state, and local levels. Indeed, these events suggest an officialdom apparently responsive to expressions of public opinion. The answer to "how to do it" in 1969–1971 appeared to be the use of rather old-style democratic politics—massive pressure on elected officials.

Professor Sax does have a point, of course, in his analysis of the court's role in environmental policy development and implementation. And it is surely legitimate to make the case for alternative methods to enforce identifiable public rights. Citizen groups may indeed find court action a productive method for enforcing environmental standards. But Sax overstates his case against the present system, seriously detracting from the very goals he seeks to achieve in the book and limiting its utility for the scholar.

Saltonstall's book is what it purports to be—a citizen's guide to environmental problems and suggestions for "each person doing his little bit." His is one of the few books in this genre to discuss environmental law and to provide a list of environmental agencies, private organizations, and congressional committees. On the other hand, the legal analysis is weak and the list incomplete. Further, attempting to clean the water and clear the air by altering toilet habits ("turn off the faucet when you are brushing your teeth") and spare-time activities ("plant a garden or tree") is futile. Each individual confident he is "doing his part" may in fact prevent the active citizen pressure needed to enforce environmental controls.[9]

Economic Analyses

The economists have been analyzing the costs and effects of pollution and policy alternatives for some time. Of the contemporary literature on the subject, *Pollution, Property, and Prices,* by J. H. Dales is of considerable interest to the political scientist. Professor Dales places policy choice in the context of production and consumption—identifying some of the social and economic costs of waste disposal.

> The main purpose of this volume . . . has been to put pollution into the perspective of a society-wide problem. In the vast majority of pollution problems, it seems to me, we pollute each other, so that it makes little sense to classify people into polluters on the one hand and pollutees on the other (p. 101).
>
> The only real question is how much you and I are willing to pay in order to reduce pollution (p. 100).

Dales illustrates how economic analysis helps to "decide on the best way of implementing a policy once it has been chosen." He charges the politicians with the responsibility of deciding what the public wants. When they judge it socially desirable to control pollution, then "they must rely on advice from others if they are to come to a wise decision" on implementing policy. Dales identifies three "strategies of implementation: regulation; subsidization; and some system of charging for user rights." To an economist, neither regulation nor subsidization can "possibly be efficient." Rather,

> It is suggested that transferable property rights be established for the disposal of wastes. Because transferable . . . property rights always command an explicit price, the establishment of such Rights makes it easy to establish a market in them . . . the market automatically ensures that the required reduction in waste discharge will be achieved at the smallest possible total cost to society (p. 107).

The principal difficulty with this solution is its impracticality for reducing multiple-source pollution. Here Dales recommends combinations of the three strategies, plus taxation.

This is one of the few books reviewed here which realistically identifies an aspect of the environmental issue, brings social science method, theory, and knowledge to bear on the problems raised. The author does not permit his own anxiety about pollution to interfere with his responsibilities as a scholar. The book might well serve as a model to be used by political scientists analyzing the environmental issue-area. Dales's conscious use of economics contrasts sharply with the style of the political scientists reviewed here (see below) for he turns first to his discipline in analyzing a new problem; our colleagues tend to start afresh with each new problem.[10]

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[9] One of the more practical "how to do it" books is *Ecotactics* (New York: Ballantine Books, 1970).

The Politics of . . .

To this point we have been primarily concerned with assessing contributions to the base of knowledge for analyzing issue emergence, issue characteristics, and policy alternatives. Not unexpectedly, perhaps, few political scientists are represented in the works cited so far. We may rightly expect them to be writing "the politics of . . ." books, however.

While the other books can be assessed for what they add to our substantive knowledge; the politics books should be evaluated for what they contribute to our knowledge needs in categories four and five—description and analysis of institutional bases of action, and policy development and implementation. In that evaluation, it seems reasonable to test whether and how the authors apply accumulated political wisdom and integrate their findings, if any, into what has gone on before. Surely, if these things do not happen, we cannot carry policy analysis very far.

One of the quickest ways to hit a new market for books is to collect relevant articles in a reader. We have such an example in Walt Anderson, ed., *Politics and Environment: A Reader in Ecological Crisis.* While the readings included in this book are not limited to process and policy matters, presumably we are to learn something about politics in using these materials. The author begins, however, by saying that political scientists have ignored the subject of his book. They ". . . have somehow managed to avoid confronting as a totality the most pressing complex of political issues of our time—those which can be called environmental" (p. 1). How can one organize collected readings then? Simple enough—include works on environmental politics by journalists, public officials, and others who do write on environmental politics. Since very few people at all write on the subject, however, the next step for producing a timely collection is to expand the meaning of politics:

> . . . we must realize that politics is the making of decisions by human beings about the manipulation and utilization of the natural environment; it is an interaction among life forms which directly or indirectly affects other life forms. Increasingly and inevitably, politics *is* ecology (p. 9).

Now it is possible to include all materials ever written (and, despite the title, the new definition makes the book little different from other "scope of the problem" books). Anderson limits himself to the familiar environmentalists—Paul Ehrlich, Rachel L. Carson, Barry Commoner, Stewart L. Udall, Lewis Mumford, etc. As expected, political scientists do not make the list.

Is political science really so barren? A response to that question depends on what one is looking for in the literature. Professor Anderson apparently searched for studies by political scientists which confronted the environment "as a totality." Not surprisingly, his search was in vain. And yet he did not insist on that standard for other works included. Practically all of his selections treat special aspects of the environment—not the totality. Lynton Caldwell, Vincent Ostrom, Grant McConnell, Dean Mann, Norman Wengert, James L. Sundquist, Philip Foss, among other political scientists, have been studying aspects of environmental politics for some time. Nothing from their many published works is included in Anderson's volume.

Or suppose in organizing a set of readings on this issue one asked, "What do political scientists know that would be helpful in understanding politics and the environment?" Suddenly many names come to mind: e.g., Murray Edelman and Emmette Redford on regulation; David Truman on group organization and action; Don Price on science and technology; Theodore Lowi and Robert Salisbury on policy effects; Morton Grodzins and Daniel Elazar on federalism; Aaron Wildavsky on planning and budgeting; or the work of political economists like Charles Lindblom, Anthony Downs, and

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[10] Other recent economic treatments include the popular collection of readings edited by Marshall I. Goldman, ed., *Controlling Pollution: The Economics of a Cleaner America* (Englewood Cliffs, N.J.: Prentice-Hall, 1967); the recent reprinting of Harold Wolozin, ed., *The Economics of Air Pollution* (New York: Norton, 1966), which contains an excellent case study of policy development at the local level; and two Resources for the Future publications—Orris C. Herfindahl and Allen V. Kneese, *Quality of the Environment: An Economic Approach to Some Problems in Using Land, Water, and Air* (Baltimore: The Johns Hopkins Press, 1965); Allen V. Kneese, Robert U. Ayres, and Ralph C. d'Arge, *Economics and the Environment: A Materials Balance Approach* (Baltimore: The Johns Hopkins Press, 1971). See also specific readings in the many collections cited in other notes.

Mancur Olson. And it is always worth recalling that John M. Gaus's lecture, "The Ecology of Government," was delivered over 25 years ago. It may well be that these scholars do not confront the totality of the environmental issue. Many attempt to confront the totality of politics, however. I think careful selection among their works, in combination with readings from political scientists who have worked more directly on the issue and from the ecologists and other scientists who define the scope of the issue, could easily result in an impressive volume on "politics and environment."

Professor Leslie L. Roos, Jr., meets many of the criteria for a "political science" reader on the environment in *The Politics of Ecosuicide.* Making no apology for what his discipline has to offer in analyzing the environmental issue, Roos is guided in compiling selections by three sets of questions:

> (1) How are decisions about environmental problems made at the national and at the state level?
>
> (2) What is the role of analysis in selection of the "proper" environmental policies? Might different types of analysis lead to different conclusions?
>
> (3) What are some methods for bringing about change in the ways Americans treat environmental problems? What institutional and behavioral patterns might be altered to facilitate such change? (p. 1)

He has been more successful than Anderson in finding material by political scientists. And where readings were not available, he was able to get original articles contributed to the volume.

The sections on public opinion, the role of analysis, and the environment as a political issue, contain the most useful new material. In including Allen Schick's article, "System Politics and Systems Budgeting," Roos illustrates that an article does not have to direct full attention to environmental politics to be of value in analyzing that topic. And the chapter by Robert Dorfman and Henry D. Jacoby seeks to combine political and economic factors in a benefit-cost mathematical model of public decisions (using a hypothetical water pollution case). Those stressing the economics of pollution in policy courses will find this piece a superb complement to Dales (reviewed above).

The book does have shortcomings, judged by the criteria employed here. The sections on institutions and the bureaucracy are distressingly small (together constituting only one-eighth of the book). There is little reliance on the many scholars mentioned earlier for setting a broad framework of analysis (though many of the individual pieces draw from those scholars). And the title is unfortunate, in my judgment. It is misleading, unnecessarily so given the intended audience.

But Professor Roos deserves plaudits for a really fine effort. He has selected carefully and purposefully, and his introductory comments are most helpful in integrating the selections.[11]

The Politics of Pollution, by J. Clarence Davies, III, is an original work. Davies draws primarily on his experience as a budget examiner of federal pollution programs and, secondarily on his skills as a trained political scientist to produce a valuable survey of the political dimensions of air and water pollution problems and policies before 1970. I want to emphasize the book's qualities. It does provide description and analysis of institutions and policy—including excellent chapters on the complexities of setting standards and getting compliance. And though the book was published just before the major organizational changes and policy developments in 1970, the background material and some of his speculations on future policy are helpful to student and researcher alike.

It should also be said that Davies does not claim political science has nothing to offer the analyst of environmental issues. Indeed, it is in part political science training that permits him to explain pollution politics. Still he makes only the most limited effort to set his findings in the context of what has gone on before. It is as though his colleagues had offered him nothing to work with in analyzing this set of issues, and no place to go with his findings. I am confident Davies does not believe that to be so. But for whatever reason, he chose not to let existing theories, generalizations, and frameworks guide his analysis. Economist Dales relies primarily on his discipline, political scientist Davies relies primarily on his experience. We profit from both but Davies had the chance to combine the enormous benefits of both.[12]

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[11] Other readers with sections or selections on politics include George E. Frakes and Curtis B. Solberg, eds., *Pollution Papers* (New York: Appleton-Century-Crofts, 1971); Fred Carvell and Max Tadlock, eds., *It's Not Too Late* (Beverly Hills, California: Glencoe Press, 1971); and several of the listings in footnotes 3 and 4.

[12] Of course the Davies volume is "super-analysis" compared to James Ridgeway's book, *The Politics of Ecology* (New York: Dutton, 1970). Advertised as a "nationwide handbook for survival," the book is actually a very superficial account of some aspects of water pollution (principally oil spills). It is one of the worst examples of "instant books" capitalizing on a new issue.

Summary

I have sought to evaluate the contributions of these 14 books to our knowledge needs for understanding environmental politics. Overall, I am not encouraged by what I have found. Though many of the books may be useful for other purposes, they fall short of meeting the needs identified here. Those seeking political knowledge on environmental issues can save time by paring this list to five books: Caldwell, Dales, Roos, Davies, and the second annual report of the Council on Environmental Quality (with the American Chemical Society's report included, if needed, for an introduction to scientific aspects of environmental problems—see footnote 6).

I have also evaluated the Anderson, Roos, and Davies volumes by a standard of cumulative political wisdom. I did not begin reading them with that measure in mind. Rather, it developed as I puzzled about what was lacking—particularly in Anderson and Davies. Perhaps I am unduly optimistic, but I think we have a base of knowledge that can be applied to emerging policy issues. It may not be as refined as we would like, but at least something is there. Unless we first come to know what we know, however, we will continue to despair of our ignorance and confirm our irrelevance as each new issue emerges.[13]

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[13] It may well be that the environmental issue is simply too broad for policy analysis at this time. Matthew A. Crenson, *The Un-Politics of Air Pollution* (Baltimore: The Johns Hopkins Press, 1971), and George H. Hagevik, *Decision-Making in Air Pollution Control* (New York: Praeger, 1970), study one of its many aspects. Both are highly sensitive to the need for integrating their findings with those of other policy theorists and scholars.

BOOK REVIEWS

The Common Sense of Politics. By Mortimer J. Adler. (Holt, Rinehart & Winston, 1971. Pp. xiii, 265. $6.95.)

Audacity marks much of Mortimer Adler's writing, and no book more than *The Common Sense of Politics*. He has always believed that definite and truthful things can be said in brief compass about great philosophical subjects; and that he can and does say them. His tone is usually level and patient; he treats his adversaries with respectful seriousness. But without fatigue, he takes up, one after another, questions of moral and political philosophy and the philosophy of mind, and offers *answers*. He has some of the awesomeness of his master, Aquinas. Adler describes his own discourse merely as "not embarrassed to affirm and defend the truth of moral principles" (p. xi). But there is more than a lack of embarrassment.

We must not be put off by the writer's confidence in his own powers; or by the book's surface, which is old-fashioned. To express it condescendingly (in order to reflect some surprise): Adler's work, including *The Common Sense of Politics,* is immensely instructive. Most want to brush him aside; that would be wrong.

The aim of the present book is to set down the first principles of political philosophy. "At its very best, it consists of no more than a slim body of fundamental truths" (p. 15). These truths are arrived at when philosophy refines common sense. In political matters, common sense consists in the common response to "noninvestigative" experience, experience we all have "*without* asking a single question that calls for steps of observation especially contrived for the purpose" (p. 43). Ethics, the direct study of the good life for the individual person as individual person, rests on the refinement of a common sense which is essentially the same throughout time and in all places: ethics rests on the ineluctable continuities in the human condition. Political philosophy, on the other hand, changes as the political order changes: political common sense changes with political experience. But the only changes in experience that count philosophically are the ones that contribute to the emergence of the good society, to the perfection of political institutions (government) and the other institutions of civil society (the state). Adler believes that the three great changes so far are those embodied in the creation of Athenian democracy, the American Revolution, and the Russian Revolution. The first introduced the rule of law and made citizenship possible for some, thus destroying the arbitrary rule of one man; the second led to the gift of citizenship to all; the third announced the emergence of welfare as the highest economic ideal, and classlessness as the ultimate social ideal. Perfection is defined by the existence of a worldwide federal constitutional democratic welfare state. Only this state is adequate to the task of securing the good life for every individual in the world. Adler's assumption is that every well-intentioned person who tried to think rationally about the first principles of political philosophy would have to hold the principles Adler does.

The basis of Adler's certainty about the principles of political philosophy is found in (what he takes to be) the truths of ethics. These are as follows. The good life is the life of happiness. Happiness (a condition, not a feeling or sensation) is present when "all real goods are present" (p. 24). Some goods each individual acquires as it were on his own: goods of the body, mind, character, and personal association. Others are "mainly dependent" on the "public conditions of his life" (p. 25). These latter include liberty, safety, material sufficiency, dignity. The state indirectly facilitates the first sort, directly establishes the second sort. Goods are the satisfaction of human needs. Needs, and only needs, establish rights. Institutions exist only to satisfy needs (to allow men to pursue or enjoy happiness) and to do so by guaranteeing rights. Adler thinks it not improper to say that when the worldwide federal constitutional democratic welfare state is reached, the limits of institutional perfection will have been reached: everything required for the good life for all will be on hand. Progress thereafter would be registered only in man's "interior life" (p. 203).

The parts of Adler's system fit together in an aesthetically impressive whole (more condescension). It seems to me, however, that little in it is free of perplexity—whether it be the claim that happiness is the right name of the good life, or the derivation of rights from needs, or the presumed desirability of a single global sovereignty, or the designation of a terminus for social progress, or the trust in common sense, or the equation of philosophy with a particular philosophy. (One could quarrel with him also for what he leaves out: most importantly, the question of the *will* to live the life imagined by Adler as the good life. He makes no reference to heroism, masochism, spite, or mysticism; all these may prey, sometimes commendably, on Adler's ideal. Does he think that humane conditions of life automatically make life endearing, even tolerable?) Quarrel one must: Adler's book deserves that response. His hard thought cannot be accepted as it is presented, as a body of self-evident truth. But it can be accepted as a serious contribution to the uncompletable work of political philosophy.

Perhaps the most striking aspect of Adler's book is the effort to refute anarchism. He says that one of his main motives is to restore belief in the present value and future perfectibility of political and social institutions. He challenges those who think people could live the good life without government (and other fixed social forms). He goes so far as to say that far from being an unnecessary or necessary evil, government is an intrinsic good. It is an intrinsic good because the greatest good—the good life for all —could not be had without it. That is because a check is needed on vice, and an agency is needed to make decisions in circumstances in which reasonable men can disagree about what to do.

I doubt that Adler has made his case. Assume government at its best, by Adler's definition: uncorrupt and perfectly accountable representative government with universal adult suffrage. It would be an intrinsic good only if we could show first that it did not have a cost, that it was a pure good; and second that though it was instrumental to important values, it was instrumental by processes peculiar to government that were themselves valuable apart from the values they served. On the first matter Adler is impatient with those who see any infringement on autonomy as evil: he rejects Mill's contention that restraint *qua* restraint is an evil because restraint is pain and pain is evil. Also, he does not think that rightly inflicted punishment presents any moral problem; but punishment is also pain and pain is evil. Government rests on restraint and punishment, and on compulsion as well. Surely we would prefer a world without their necessity. We accept them *faute de mieux;* therefore the essence of government is compromised. Government at its best would still have this cost for its achievements, and a most serious one it is. No fiction of consent can remove the curse from authority.

Suppose that matter were forgotten: are the processes peculiar to government (if any) valuable apart from the ends they serve? If not, government at its best would be at best *only* an instrument: it would fall into the realm of the morally *indifferent.* What is needed here is a theory of the value of political action. If anything could save Adler's argument, it would be such a theory. Adler does not even hint at such a theory. He exhausts citizenship in voting. Voting *is* worth something, for all the failures of democratic government to be democratic. But it is not nearly enough to permit us to say that government, even at its representative best, would be an intrinsic good—except for those few who held office. Only if everybody took his turn in office (or played some substantial role in political processes) could we say that maybe government is an intrinsic good. But then we go beyond the theory of representative government.

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A Primer on Social Dynamics: History as Dialectics and Development. By Kenneth E. Boulding. (New York: The Free Press, 1970. Pp. 153. $5.95.)

"Primer" is perhaps not the exact word for a book as subtle in its argument, as allusive, and as complex in the issues it raises as this one. But Professor Boulding is one of our most humanely literate minds in the social sciences today, and in this work as in others from his pen, the author's mind and style carry us, even the most unspecialized of us in these matters, a long way. He never succumbs to the simplistic or facile, but the subjects he deals with are made surprisingly lucid. Whatever objections I may have here and there to the contents of the book, the author's learning and commitment to substance are everywhere evident.

The chapter headings indicate clearly enough both the kind of book it is and the inherent excitement of its contents: History as system; organizers of social evolution; dialectical patterns in history; nondialectical patterns in history; the Marxist dialectic; revolutionism; how history might be written; and, as a brief appendix, the case study of Japan. They are not merely headings of chapters; they are, properly, road signs for a journey that has, as few books these days have, beginning, middle, and end.

The book is polemical. The best scholarship usually is, for no matter how insistently the ultimate referent—the reality out there—may beckon, scholarship at its best is engagement— parrying, thrusting, occasionally demolishing— other scholarship. There is a good deal of this in Boulding's book, which makes for a substantial part of its excellence. The one advantage we in the academy have over the bloody amateurs who write ringingly (in the cash-register sense) about nonexistent things like "future shock" is our knowledge of what others have done, what they meant, and what their excellences and deficiencies were. Scholarship began with, not Plato, but Aristotle on Plato.

To a large degree Boulding is writing on Marx in this book, or rather on the whole set of principles concerning the nature and mechanisms of change we associate with the name of Marx. Specifically, it is to what Boulding calls dialectical philosophy—"ideologies which regard conflict as the essential process in development and therefore put a high intrinsic value on conflict, struggle, war, and revolution" (Preface)—that he directs his polemic. Not that dialectical emphases, in this sense, are without value at times in history. As Boulding says, in times when the prevailing dogmas are built around orderliness, agreement, courtesy, love, and rapt worship of the One, perspectives founded on perceptions of conflict are at least pragmatically useful. What availeth, though, when the dogmas are themselves ones of conflict all compact? To look at the mannered, self-conscious, ever more precious stylistic extremes to which our secularized, bush-league Augustines of the present day must reach if their shrillness is to be heard over shrillness, is a study in futility. If Marx were alive and young today, I rather imagine he would take his departure from the dogmatics of conflict, so richly if wearisomely the foundation stone of sociology and political science today, rather than from the dogmatics of Ricardian orderliness. That's only a guess, of course. One never knows for sure what a genuinely radical mind will do. What we do know is that Marx took on the academic idols of his day, which were those of equilibrium in the social process, and one likes to think he would today take on *our* reigning idols, which are those of conflict and disorder and their talismanic properties.

Boulding, in a sense, makes the idols of conflict his polemical point of departure. As I say, he does not dismiss conflict out of hand as an important factor in history. His major argument, nevertheless, is the superiority in history, on the whole, of nondialectical processes in the building of culture, social organization, and consciousness. In a stunningly good and simple chapter on revolutionism he makes clear, it seems to me for once and all, how absurdly inflated are all the conventional sentimentalities of the dogma of revolution. Revolution, in the political sense of the word, is what might be called the heroic dish of the whole conflict banquet circuit. If conflict is good, then revolution is better: so might the folklore of the academic mentality in sociology and political science, or much of it, be put. There is, however, scarcely a shred of evidence to support the view that the major revolutions of modern history—French and Russian foremost—have done other than *set back* the processes of economic and social improvement which were well under way, powerfully under way, until the Jacobins and Bolsheviks, successors to the religious terrorists of post-medieval millennialism, got into the act.

I should be doing Boulding's book a disservice, however, if I left the impression that it is a tract against revolution as such. "The principal villain would seem to be the dialectical philosophy" (p. 92). So writes Boulding. It is the sustained, sometimes brilliant, often wise, always knowledgeable analysis of the whole dialectical consciousness, born of religion and its envisagement of the world as one of conflict between Good and Evil, that gives the book its extraordinary value.

What does Boulding offer as substitute for dialectical philosophy? In general, developmentalism, with its processes of genetic causation, cumulative continuity, and lineal directionality. Within this broad pattern, Boulding identifies growth of knowledge, social exchange, economic development, and what he calls the integrative system as key elements or mechanisms. It is here, with no diminution of respect for Boulding's learning and insight, that I would myself be obliged to raise a question or two. Boulding thinks of these, if I read him correctly, as counter-mechanisms to conflict, including revolution. Actually, they are not. They are descriptive rather than analytical terms, it seems to me. Broadly speaking, this is true of developmentalism as a whole. However false the dialectical philosophy may be—and I regard it as false—it at least seeks to provide a *causa efficiens*. The trouble with developmentalism is its metaphoric assumption of the normality of exactly what we need explained: change, mutation, transformation in social and cultural history.

Taking Western ideas of change in their aggregate, from the Greeks down to contemporary Marxists and functionalists, it is impossible to miss, if we look closely, the common *correspondence* of developmentalism and dialectical philosophy. Few developmentalists have been revolutionists, as Marx was. But a great many developmentalists, from Heraclitus to Darwin, Marx, and Spencer, have made conflict the efficient cause of the development—the cumulative, continuous, and genetically structured development—that was their largest perspective.

The same Marx who prescribed revolution could warn readers of *Capital* that no people can "clear by bold leaps . . . the obstacles offered by the successive phases of its normal development." Some of the arguments that went on among Bolsheviks in 1917–18, until Lenin put a stop to them, had to do with this point.

Boulding at one point observes, quite correctly, that developmentalism looks best as a philosophy of change when it is applied to large entities over long periods of times. Where it is least helpful, with its key elements of genetic continuity, cumulative and more or less linear change, and assumed directionality, is with respect to the smaller entities—specific communities, regions, and nations—and to the shorter periods of time. The reason for this—and I am not sure that Boulding sees it clearly—is that developmentalism is basically inseparable from the metaphor of growth; and it is only when we step back from the concrete, the empirical, and the temporal and let our eyes roam over great vistas of the past that the metaphor of growth can seem really pertinent. Even then, however, it tends to be merely descriptive, at worst sterile taxonomy, with faith in growth required to link the individual snapshots into a moving picture, into what we are prone to call "dynamics."

Biologists have been much more successful than social scientists in relating the concrete variations and mutations revealed by genetics to the study of comparative structure, to the grander vistas of evolution. Perhaps this is the consequence of what Ernst Mayr, in his classic studies of speciation, has referred to as the biologists' liberation from the typological thinking that tended to dominate biology until early in this century. The social sciences tend still to be heavily typological in their thinking. The continuing vogue of Marxism, of social systems analysis, of structural-functionalism, indicates that we are not much less typological in our approach to the problem of change than were our forefathers all the way back to Aristotle's *Politics*.

If we move from the social sciences and their typological systems, their passion for unified theory with respect to statics and dynamics, over to the writing of history, to historiography, we do not, alas, get much help. For history remains almost as dedicated to its own metaphor of genealogy as the social sciences are to the metaphor of growth. Boulding has some splendid observations on the writing of history in our time, but I think he misses the point that what is chiefly wrong with historiography is not just the technical problems of bias, management of data, objectivity, and the like—ever the concern of courses in historical method—but is instead the nature of historiography itself. This nature, which was old when Thucydides gave it lasting sanction with his paradigm of the Peloponnesian War, is one of taking multiform, multilinear, and multidimensional reality and then forcing this reality into a uniform, unilinear, and unidimensional genealogical pattern—that is, narrative history. Events beget events, acts beget acts, and personages beget personages in the time-honored fashion of the epic. Instead of the chosen people, it is the chosen nation or civilization or economy or religion. The flow of time (Greenwich time, naturally) is made identical with the flow of events, acts, heroes, and whatever else historians interest themselves in.

All of this is unexceptionable in the novel, the epic, or the play, and as long as historiography is regarded as simply another art form (with somewhat more attention given to verisimilitude) its practitioners cannot be faulted. But if what we are interested in is a degree of knowledge of the mechanisms of social change comparable to what geneticists have revealed about biological change, historiography is of no more help to us than developmentalism. Each must have its unified structure, and whether this structure be one suggested by growth or genealogy, we are still prisoners of metaphor.

Professor Boulding concludes his book with an interesting appendix on Japan as a case history. I assume it is a case history in behalf of his plea for developmentalism, though I cannot be sure inasmuch as case histories individually presented never seem to me to tell us much except in the realm of the world of art—the painting, the sculpture, or the poem. If Professor Boulding—who is, among other things, a talented artist—wants to leave his limning of Japan at this—that is, an art work—I will not demur or cavil any more than I would at the limning of Japan by a Lafcadio Hearn. But if he is trying to tell us something about the dynamics of change, something comparable to what the geneticist tells us about the dynamics of change in the biological world, then I must urge that he get back to the drawing board.

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The Public Interest and Individual Interests. By Virginia Held. (New York: Basic Books, 1970. Pp. 248. $6.95.)

This is a literate, gracefully written book, which is easy to read and to follow. The author's perspective is objective, her manner and judgments are temperate, and her treatment of a wide variety of predecessors (writing directly or obliquely on public interest theory) is invariably civil. She has appraised—fairly and competently—writings separated by a considerable amount of time and geography, to say nothing of language and culture. After a brief introduction to the recent literature on public interest theory (produced mostly by Americans, and otherwise by Englishmen, during the period 1959–66), Dr. Held explains her approach. She empathizes best—and most often—with modern analytical legal positivism; and she focuses on the justification of claims in behalf of, and upon the reconciliation of conflict between, individual interests and the public interest.

Notwithstanding the seeming emphasis of her book's title, the value that preoccupies Dr. Held is individual interest(s); the public interest serves as a foil to make possible the validation and adjudication of conflicting claims of individual interest. Indeed, working throughout the book on the premise of methodological individualism, she achieves in her conclusion (p. 197) a position of really quite extreme individualism whereby—if I understand her correctly—a minority of (any) one can stalemate a plurality of 200 million other individuals, leaving preference between these two opposing interests to the determination of a vague (and in this book, undefined) ethical supernorm. Each individual is defined (pp. 36–37) to be a member of a set; a group is simply a plurality of individuals; while the universal set of individuals (apparently limited, but in an unspecified manner, by space and time) constitutes the "polity" (which eventually, near the book's end, becomes synonymous with "the political system"). As the author remarks, "The term 'individuals' traditionally and often refers to individual persons, but may, as indicated, refer to groups or suborganizations of the polity." The interests of (such) individuals are considered to be clear and univalent: "I shall have to assume that 'x is in the interest of I_i' and 'x is not in the interest of I_i' are not being asserted simultaneously by the same person or authority of the same x's and I_i's" (p. 33); and thereby the ambivalence and ambiguity of self-interest (to say nothing of group-interest), which so frequently can be observed to obtain empirically, are assumed away and cannot enter into the calculations of Dr. Held's theory (or analysis).

It is at least conceivable that one might endeavor to understand the meaning of concepts of public/individual interest on the basis of empirical evidence about how people act—what they do, and why, and with what consequences—in attempts to invoke, justify, and reconcile claims related to these concepts; one might even premise recommendations about what people ought to do, in such circumstances, on the basis of some systematic knowledge concerning what they can be observed to do without the guidance of such recommendations. But in this book empiricism does not even extend to the analysis of claimant language behavior in relation to the central concepts; the author's purpose was to deal instead with the secondary evidence of statements by other writers about these concepts, plus a few (strictly logical) statements of her own. She undertakes, in her own words, to "grapple" with the term "public interest," which she purports to do by making a *verbal* analysis of the logic of linguistic relationships. Dr. Held's approach is, from a methodological point of view, quite clearly that of modern philosophy rather than that of modern political science.

The bulk of the book presents a typology of public interest conceptualization. In each of three chapters the author initially describes the views of the traditional philosophers (her phrase) whom she deems most meet for exemplification of each type; following which she describes the corresponding views of contemporary writers, similarly selected; finally, she proffers a "problem" in the form of a case study that illustrates inconsistencies and inadequacies of the viewpoint under analysis. "Preponderance" theory (from Hobbes and Bentham through neo-Bentleyans and welfare economists) describes how majorities aggregate individual interests and validate (in their own name) governmental decisions. But according to the author there is no way to demonstrate that the majority is ethically equivalent to the public; and empirically, majoritarian decisions rest upon mere superior force. The weakness of preponderance theory is that it leaves no room for any transcendent ideal of public, in the sense of *res publica;* neither can it entertain any concept of public interest that goes beyond mere identification with majority will. Common will theorists from Rousseau to Buchanan and Tullock take the opposite tack, and eliminate individual interests by having them gobbled up in an all-embracing bearhug of common interest, which brooks neither deviation nor dissent. Unitary conceptions of public interest (Plato, Aristotle, Aquinas, Hegel, Marx, and such contemporaries as Cassinelli and Flathman) are supposed to be different from those of common interest, but they seem to be equivalent in their empirical implications. Even from a logical point of view, both perspectives are said (pp. 44–45) to rule out the validity of the assertion together of the following pair of statements: "x is in the public interest" and "x is not in the interest of individual I_i." The main difference seems to be that by definition there can be no conflict between the common interest and any properly understood individual interest; while under the unitary theory, conflict is possible, but it is capable of only one kind of outcome, and that in favor of the superordinate right of the collectivity.

Dr. Held seeks to make up for the various shortcomings of the three predominant viewpoints by suggesting what she hopes may become recognized as a more adequate explication, her own alternative conceptualization of the public/private interest controversy. Thus her concluding chapter is cast in the context, and somewhat self-consciously in the lingo, of political systems theory (in the footsteps of Easton, and Almond and Powell); surely this is a most commendable endeavor to articulate her concerns from the modal theoretical standpoint of the political science profession during the 'sixties. (Her concerns—none of which can be said to have been tributaries to the mainstream of political science theory during the past decade—include classical political philosophy, modern analytical legal positivism, and econometric decision theory.) The political system is important because only within it can the public interest be asserted "since it presupposes such a system to provide a decision procedure between rival claims of what is or isn't in the public interest" (p. 173). The political system (e.g., the universal set of individuals) is itself anthropomorphized, indeed *individualized,* because "reasonable expectations may be that it will assert, as will any individual, first a claim to survival, later to growth, and that, with the progress of awareness and organization, it will become capable of choosing which among various possible futures to try to bring about" (p. 174). Such a personified polity validates public interest claims by adjudicating rival demands, on the basis of whichever is approved by "greater political authority" (p. 176). Although, as we have seen, no judgment concerning the public interest can be valid outside the political system whose decision procedures validate claims about it, nevertheless "judgments concerning individual interests may be independent of judgments concerning the public interest, and the political system can itself be judged in moral terms" (p. 183).

This turns the polity into a court of first instance, because its "authoritative" "validations" of the public interest are that only for consenting adults; any "majority of one" (and I do not quote the *author* in my use of this phrase) can challenge the polity simply by disagreeing; and then, in the fashion of *John Doe* versus *The People,* both parties litigant have equal status in the eyes of the author, because "any dispute between them can only be settled by going beyond the systems of both, even when a preponderance of individuals may give weight to one side or the other" (and here I *do* quote Dr. Held, at p. 188). And that implies that there must be a "more inclusive system, containing both a political system and separate individual systems, [which] . . . might be, for instance, the ethical system of a society" (p. 188). One anticipates that the discussion will turn next to the decision-making processes of ethical systems and the validating propensities of ethical authorities (if I do not strain the metaphor too much, by insisting that it remain within the same realm of discourse as that for political systems). But here we run into a blank wall, as on the very next page where the author proclaims, "I shall not begin to discuss the nature of ethical systems, nor possible decision methods for rival claims between political systems and individual systems."

So where does this leave us? With nonauthoritative authorities validating public interest claims that are binding upon no one, and without even an inkling as to how to cope with political dissent, beyond the suggestion that this raises an ethical question about which consensual judgments might possibly be reached by coteries of "independent and impartial scholars" whose opinions would be accorded the same kind of respect given (as the author thinks) to the judgments of natural scientists (within their own fields of appropriate expertise, of course). In her own words, "a conflict between a claim in behalf of the public interest and a claim in behalf of one or several individual interests would be resolvable at the ethical level, and might be voluntarily accepted by the rival claimants" (p. 191).

Apart from a few more pages of applications, and an appendix, Dr. Held quits at this point and no doubt I should do the same. I really don't wish to say anything ungracious about a book whose author has manifested such equanimity toward others, but I must register my profound skepticism concerning the utility of any solution (to the problems of public-interest theory) which rests upon the personification of political systems (what price sovereignty?); which renders political systems so atomized that we are all cast back into the state of nature from which civilized mankind was to be protected by

Hobbes's sovereign (or, if a more empirical example is to be preferred, the chaos that lay beyond the walls of the *polis* two and a half millennia ago); and which invokes as its *deus ex machina* a community of dispassionate social scientists and humanists. If I may paraphrase a leading spokesman for the ideology of legalism that Dr. Held finds so attractive, "Some who profess belief in the [Public Interest] read literally what others read as allegory or metaphor, as they read Aesop's fables. [Political] symbolism is even used by some with the same mental reservations one has in teaching of Santa Claus or Uncle Sam or Easter bunnies or dispassionate [professors]." I wish that I could conclude otherwise, but nothing in Dr. Held's book persuades me that public-interest theory has changed much from what it appeared to be a decade ago.

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Essential Works of Socialism. Ed. by Irving Howe (New York: Holt, Rinehart and Winston, 1970. Pp. 406. $12.95.)

In his Introduction the editor stresses the "enormous" difficulties in compiling this anthology. Indeed, given the admitted vagueness of the concept of socialism and the myriad books, pamphlets, and articles written by "socialists" of all hues, the enterprise of culling "essential works" out of this accumlation probably was doomed in advance. Had the editor approached the material with a clear and rigorous framework—a requirement more easily suggested than accomplished—the reader would at least have been less confused and more stimulated. The criteria of selection guiding Howe were, however, too broad and indefinite: to choose "texts . . . that would be valuable both as political reflection and historical evidence . . . writings [which] . . . matter in significant ways, sometimes as guides to opinion and action, sometimes as inducements to argument and disagreement" (p. 1). He even decided to include writers "who may not quite be socialists themselves," a choice which, if pursued consistently, would have brought the huge additional literature on problems of socialism written by more or less sympathetic nonsocialists within his range of selection.

On the other hand Howe applies—though again not consistently—a principle of exclusion intended to save the concept of socialism from its detractors: "Apologists for both Stalinism and such post-Stalinist authoritarians as the Maoists and Castroists" who, in his view, have "blemished the name of socialism" (p. 2) find no place in this anthology. In spite of this disclaimer, Howe does devote a section of his volume to "the Bolshevik tradition" and includes selections from Lenin, Trotsky, Bukharin, *et al.* He defends himself against the charge of inconsistency with the argument of "intrinsic political value," having just conceded that the excluded "authoritarians" are "obviously important and their ideas merit study;" and—more interestingly—"because there are also democratic components in their thoughts" (p. 2). Were Howe to pursue this latter point he would have to concede that the relation between socialism and democracy has never been an unequivocal one, particularly if democracy is defined in terms of parliamentarism and the existence of a multiparty system. Starting with Marx and Engels there have been hardly any major socialist thinkers—including Luxemburg, Kautsky, and Bauer—who were unambiguously committed to what Luxemburg derisively called "parliamentary cretinism." Is it not misleading to include her famous criticism of Lenin's and Trotsky's dictatorial methods but to exclude her approval of the use of force against "the class enemy?" Or to break off the selection from her reply to Bernstein (p. 120) just before her reference to the "chickencoop of bourgeois parliamentarism" which Bernstein had considered—falsely, in her view—the "appropriate organ for the most tremendous world-historical transformation?"

The recent development of an "extra"-or "anti-parliamentary" movement on the Left thus represents, in my view, the return to an old socialist tradition merely temporarily interrupted by the liberal consensus of the postwar period. By dogmatizing the relationship between socialism and parliamentary democracy instead of perceiving it as problematical, Howe fails to explore some of the most significant controversies; and he scarcely confronts the very real problem giving impetus to the socialism of the New Left: the apparently built-in incapacity of overcoming the capitalist system by means of parliamentary reforms. However open to criticism Herbert Marcuse's attack on liberal tolerance and the alleged "totalitarianism" of Western democracies may be, his writings surely are "essential" for an understanding of the present-day discussion among socialists who have moved beyond the Stalinist experience and once again question the values of parliamentary democracy which Howe postulates.

Although it is easy to quibble with the specific selections made by the editor for a volume of this sort, I do believe that there are some objective grounds for my impression that his choices do not effectively guide the reader to the major topics which have dominated socialist controversy since the mid-'sixties. Thus the "Young Marx" of the "Paris manuscripts" with his emphasis on "alienation" is unrepresented. It would have been meritorious—and indeed essential, particularly for the American reader unfamiliar with the philosophical Marxism of Central Europe—to include passages from Lúkács' "History and Class Consciousness" as well as from the writings of the so-called "Frankfurt School" (Horkheimer, Adorno, Habermas). The attempt to fuse psychoanalysis with Marxism—prominently reflected in the work of Marcuse, Fromm, and Wilhelm Reich—surely should not have been totally neglected. Missing also are selections from the writings of Serge Mallet, Ernest Mandel, and André Gorz, all of whom have made serious and influential attempts to adapt Marxist economic and sociological categories to the analysis of contemporary capitalist systems. Also striking is the absence of the entire anarchist tradition within the socialist camp. Although Marxists' attacks on anarchist positions are cited in several instances (Plekhanov, Lenin), thinkers who articulated anarchist ideas (Proudhon, Kropotkin, Sorel, Goldman, etc.) are not represented, in spite of the significant role which anarchism has played in the theorizing of the New Left.

One additional criticism: I find it annoying that in most of the introductory notes provided by the editor neither the precise edition nor the pages of the original source from which the selection is taken are stated. The reader wishing to go to the original is thereby severely handicapped. In spite of its many very interesting selections, the question remains what utility such a big—and expensive—volume has, and for what specific group of readers it is intended.

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Political Conflict: Essays in Political Sociology. By Morris Janowitz. (Chicago: Quadrangle Books, 1970. Pp. xii, 271. $2.95.)

A scholar who collects in book form a series of papers written at different points in time and addressed to different audiences runs the risk that his readers will not discern a unified theme and will consider the work a mixed bag rather than an integrated contribution. On the whole Janowitz has managed to avoid this risk, although he has not been completely successful.

Despite the diversity of topics discussed, a set of underlying ideas links the different essays. Janowitz is critical of a mode of sociological reasoning and analysis in which political phenomena are seen as being mainly epiphenomenal. Against a view that considers processes of social stratification to be the heart of the explanation of political phenomena, he urges an "institutional" approach in which formal and informal political organization, as well as poliltical leadership, "are seen as relatively independent and voluntaristic elements of political change." Janowitz wishes to approach political phenomena with an eye to their distinctive characteristic rather than attempting to dissolve their peculiarity by way of reduction to alleged subjacent dynamics. In this view, the political sphere has at least relative autonomy, and is not simply a social byproduct.

This analytical focus on the peculiarities of politically relevant institutions informs most, though not all, of these essays. A survey of the study of "Mass Communications and the Political Process" stresses that such study cannot proceed fruitfully unless one recognizes that the media are in fact crucial instrumentalities for both social control and social change and not simple reflections of sociopolitical factors. A fine comparative study of "Social Cleavages and Party Affiliation" in Germany, Great Britain and the United States dissents from the view that continued economic expansion will eliminate political cleavages so that the politics of the middle-mass will be devoid of ideological character. By pointing to persistent and even increasing sources of cleavage in regional, linguistic, and religious factors, as well as in foreign affairs, Janowitz challenges the "end of ideology" thesis; he also identifies those institutional forces which, despite a certain leveling of the stratification pyramid, are fostering the emergence of new political issues. New types of political leaders who utilize these cleavages, even when they attempt to overcome them, will shape the polity into novel patterns.

In the chapter on "Military Elites and the Study of War," the author, who is, of course, the foremost sociological analyst of modern military institutions, is at pains to establish the distinctiveness of these institutions. Even though war making might be profitably studied as a special case for the application of a general theory of social conflict, Janowitz argues, it is still necessary not to overlook the particular features of military organizations. Even though modern military institutions must be seen as responsive to a variety of pressures coming, *inter alia,* from the technological and organizational sphere as well as from changing modes of indoctrination and changing relations to the class structure, it behooves the analyst to consider the military establishment as a distinctive totality, not as a mere reflection of underlying forces.

Janowitz develops a suggestive typology of political-military elites. He distinguishes the *artistocratic model,* in which political and military elites are socially and functionally integrated, from the *totalitarian model,* as in Stalinist Russia and Nazi Germany, in which a revolutionary elite of relatively low status controls the military professionals and destroys their organizational independence. Janowitz also describes a *democratic model,* in which civilian control is exercised through parliamentary institutions while the military, guided solely by professional standards, is being insulated from political contentions; in a second pair of comparisons he distinguishes between this democratic model and Lasswell's *garrison state model,* in which civilian supremacy over the military has been so weakened that the military exercises effective political and administrative power. The first two types conceptualize modes of civilian-military relationships in historical experience, whereas the last two are merely goals that certain political decision makers strive to attain. It would be interesting to have the author assess how at this point in time, fifteen years after this essay was written, he assesses the chances of implementing the democratic, as against the garrison state model. He might be a bit less sanguine now than he was then.

Space does not permit more than an enumeration of the topics covered in other chapters. Janowitz discusses "Social and Political Consequences of Social Mobility," "Civil-Military Relations in the New Nations," "Patterns of Collective Racial Violence," and "Native Fascism in the 1930s." Still another set of papers deals with the methodological dimensions of political inquiry and with relations between sociological analysis and social policy. In this regard, Janowitz contrasts an enlightenment model of social science and an "engineering model," which sharply distinguishes between theoretical and applied research and views the applied scientist as the doctor who cures society or as the engineer who adjusts it. He prefers the enlightenment model, which assumes that "the sociologist recognizes that he is interacting with his subject and a variety of publics to which he must be responsible" (p. 251) and, where "his work has an impact on himself, and his findings influence his subjects and his public in an ongoing fashion" (p. 251). I have some doubts about whether this distinction is sufficiently sharp-edged to classify unambiguously the heterogeneous tribe of contemporary sociologists, but it remains true that Janowitz's work is a major contribution to sociological enlightenment.

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The Challenge to Systems Analysis: Public Policy and Social Change. Edited by Grace J. Kelleher. (New York: John Wiley & Sons, Inc., 1970. Pp. 150. $9.95.)

This book represents Volume 20 of the Publications in Operations Research (Operations Research Society of America), edited by David B. Hertz. Given the current intolerable confusion over labels involving the concept of "systems," this additional information is necessary to alert us to the fact that the "systems analysis" of the title is the more narrowly circumscribed, quantitative, and applied brand, and not the rather distantly related "systems approach" as found in the works, for example, of Deutsch, Easton, and most other academic social scientists.

"The Challenge" of the title refers to the attempted application of this systems analysis to major social problems and public policy. And challenge indeed it is: ". . . across-the-board urban and suburban problems, pollution, safety, communications, education, population, and food."

Part I consists of five introductory chapters devoted to short histories of operations research and systems analysis, and expression of high—but tempered—hopes that these tools can meet the challenge. The authors include the President of Lockheed, two members of IDA, the Director of M.I.T.'s Operations Research Center (and pioneer in that field), and a Congressman. The remaining ten chapters, making up Part II of this small book, are likewise written largely by nonacademicians, that is, by people involved in public offices, agencies, or institutes directly faced with the social problems "on a day-to-day, shirtsleeve basis"—as Mayor Henry W. Maier of Milwaukee puts it in his chapter on "The Troubled City."

It is not really possible to comment on any of these short, substantive chapters individually, in terms of the main theme of the book. In one or two cases, "systems analysis" is not discussed at all, and in most others only a gesture is made toward either the application of such an approach, or even the delineation of the social problem in such a way as to suggest seriously its applicability. It would be going too far to say that some of these authors are grasping at any straw, or that they have been sold a bill of goods.

For whereas operations research or "systems analysis" clearly has been effective against some kinds of problems, and promising for others, it equally clearly faces difficulties when it comes to social and political problems. First, it requires a clear, precise definition of objectives or goals, against which alternative paths or solutions may be assessed ahd optimized. Though this precision may be possible for the goals of a military hardware system, it is all too often hard enough to agree on the mere definition of a sociopolitical problem (a more revealing term than simply "social problem"), to say nothing of what its goal solution might look like. Second, all the important components or variables and their interrelationships must be clearly specified if solutions are to be found or goals realistically attained. In other words, the relevant system, its internal structure and dynamics, and significant environmental interchanges must be ascertained. It appears doubtful whether we are yet able to specify adequately the systems relevant to most sociopolitical problems with the precision possible with the military systems that "systems analysis" evolved to handle.

Third, since this kind of systems analysis is a "cost-benefit" type of analysis, usually involving alternative mixes of different values of the relevant variables, we face the tough problem of agreeing on some standard (dollars?) against which human values and social "utilities" can be compared. (A felicific calculus of an "acceptable" level of a few million unemployed against a certain level of business investment seems only slightly less distasteful than Kahn's malefic calculus on thermonuclear war.) And last, "systems analysis" implies a degree of quantification and availability of mathematical techniques that are unrealistic, at least at present, as far as sociopolitical problems are concerned.

In general, then, we can argue that as a system becomes more "humanistic" and less material, more open and less confined, more democratic and permissive and less authoritarian and constrained, the main criteria for successful "systems analysis" become less attainable or even less pertinent. More often than not, this approach lacks two central dynamic elements. It does not systematically consider that changes in technologies, in cultural variables, in environmental constraints, and especially in the very goals of a social system, render the initial system out-of-date and vitiate the problem-solving activities before they have concluded. This means that any effective model must embrace the morphogenic features of social systems. Second, "systems analysis," especially as it shows up in this book, operates in a political void, forgetting that sociopolitical problems involve finding solutions for at least two major dimensions: the social problem itself, and the "political problem" that may act to promote or hinder it. And more and more of us are beginning to see the political problem as much the more difficult one.

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The Legacy of Max Weber. By L. M. Lachmann. (Berkeley: The Glendessary Press, 1971. Pp. ix, 150. $5.50.)

These sensitive essays flow from a social behaviorism akin to Max Weber's, seeing human society not as a system which acts in its own right as an autonomous entity, but as the sphere of strategies pursued by individuals, alone or in concert. Professor Lachmann's express purpose was to replace the ideal type as the central concept in Weber's theory of action, to develop a general theory of institutions from fragments in Weber's writings, and to explore alternatives to Weber's relativism in the sphere of political behavior.

All social science, but economics in particular, according to Lachmann, can benefit by dropping the view that economic behavior is a field of forces moving toward equilibrium, and by returning to the view advanced by Weber that human behavior is distinguished by the plans men pursue and the unintended consequences that may follow from their plans or from conditions outside their plans. Lachmann proposes to analyze human action in terms of plans (estimated on the basis of *verstehen*) and unintended consequences; such an analysis constitutes the praxeological method.

The praxeological method, or analysis of behavior in terms of plans and unintended consequences, poses the problem of Professor Lachmann's second essay. How is one to circumvent the rather chaotic character that human behavior displays when viewed as a field of endlessly varied, sometimes whimsical and misguided individual and collective plans and the various unintended consequences they may entail, as well as circumstances the plans fail to take into account. Such a view would seem to imply a world in which holes and gaps, strains and discrepancies were ever present. Lachmann's answer is that one must go beyond Weber to a general theory of institutions as a system of presupposed arrangements which arise either as expediencies in the course of trial and error or as a more or less enduring precipitate of past planning. The system of institutions provides the element of continuity and predictability mitigating the chaotic character of experience.

His second essay, in turn, leads Lachmann into his third, the problem of continuity and change in the sphere of political behavior. Weber's view of political life, we are told, rested on the assumption that all events in this sphere of human endeavor are determined by confrontations and encounters in which all arrangements finally reflect ratios of power. In Lachmann's view such unqualified relativism implies that even the structure of democratic institutions could erode in time. Lachmann proposes that while all political life is a matter of compromises, they vary in durability and scope and the grand compromises of interests provide long-time continuity as against the endless variations of lesser clashes of interest.

Lachmann has so sensitively appraised the contemporary relevance of some of Max Weber's notions that I find myself reluctant to criticize his formulations. He has, however, tended to "derive" Weber's position somewhat too narrowly from the economic literature of Weber's time, failing to take full account of the richness of Weber's response to historians, political scientists, linguists, students of jurisprudence, and philosophers between whom the neo-Kantian and neo-idealistic discussion was raging. Intellectually Weber was a conservative, reluctant to drop any position so long as it retained explanatory power. Hence, in the compass of his mighty personality many conflicting directions were comprised. Weber belongs to those thinkers who brood in the night as they seek resolution of the valid claims of alternative theories. For this very reason Weber would have felt, I am sure, that Lachmann has oversimplified his picture of human action and its analysis, missing some of the major problems of *verstehen* and rejecting ideal types (which are not necessarily alternative to operations of *verstehen*) which are procedures for making comparative analysis precise.

While Lachmann appears to react against Weber's intellectual conservatism, he also appears to resist his radical social realism. Weber belongs to a tradition as old as Heraclitus, a tradition that runs through such thinkers as Polybius in antiquity and Machiavelli and Hobbes in recent times. He was passionately concerned, but he always distinguished sharply between what is and what ought to be. Social forms, he believed, are cast up in the course of the ever varying strivings of men, and no single state of affairs obtains forever. Of course, he believed that some human arrangements are more enduring than others, some compromises are fairly durable. But basically he felt that social life would always have gaps, holes, obstacles, and frustrations and that even the grand compromises only endure for a time. Weber was well aware that many men found his relativism intolerable. On occasion he observed that for those for whom the spectacle of the human condition is unendurable, the doors of the church are open.

Lachmann's sensitive probing appraisal of what is living and what is dead in the legacy of Max Weber is a welcome addition to the Max Weber library; it belongs on every student's shelf.

DON MARTINDALE

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Writings on the Paris Commune. By Karl Marx and Friedrich Engels. Edited by Hal Draper. (New York, N.Y.: Monthly Review Press, 1971. Pp. 248. $7.95.)

Birth of the Communist Manifesto. With text of the *Manifesto*, all Prefaces by Marx and Engels, early drafts by Engels, and other supplementary material. Edited, with an introduction by Dirk J. Struik. (New York: International Publishers, 1971. Pp. 224. $7.50.)

When Marx uttered his famous *"Je ne suis pas Marxiste,"* he was trying to dissociate himself from people with minds less subtle than his own who, believing they were expressing his ideas, were actually distorting them. For more than ninety years since then, the process of distortion has been going on. Many of Marx's critics are guilty of it. But in the two books reviewed here, the distortion is created by two adherents.

The Paris Commune of 1871 is generally regarded in the Marxist camp as the first example of a proletarian dictatorship, hence as the first unsuccessful attempt by the working class to carry out the historical scenario sketched by Marx and Engels. Lenin in *State and Revolution* made this interpretation of the Paris Commune and its significance as the harbinger of Bolshevik revolutions a virtual dogma in the communist world; and the hundredth anniversary of the Commune was celebrated accordingly in the spring of 1971.

The facts, carefully presented by Hal Draper in this excellent collection of relevant documents, are that Marx and Engels repeatedly warned against the uprising of the lower class in Paris; they did so elaborately and in official documents published by the First International. While the Commune (and similar communes in other French cities) existed, and after it had been drowned in blood, they sharply criticized its leaders, its participants, and some of their policies. As leaders of the First International they adopted a highly ambivalent attitude: They sought to associate the International with the martyrs of the Commune, but at the same time they remained eager to dissociate the International from the mistakes the Commune had made. In its final form, Marx's Third Address to the members of the International Workingmen's Association (published in June, 1871) in fact says precious little about the Commune, having eliminated much analytical material contained in the first draft. Instead, it concentrates on the duplicity and cruelty of the provisional government in Versailles, reserving little more than vague words of appreciation for the men and women of the Commune. In later years both Marx and Engels alternated between approval and criticism, depending on the occasion of the remark. On the whole, an attitude of skepticism and criticism persisted.

The care with which Marx and Engels tried to express their attitude toward this event provides important keys to understanding their revolutionary scenario. There are other interesting matters in these commentaries on a convulsive current event, particularly the theory of the bourgeous state which Marx develops, both in his first Address (delivered 26 June 1870) and, later, in the first draft for the Third Address, written down between March and May 1870. (Interestingly enough, one of these pieces is almost totally an exercise in organization theory, which sounds rather un-Marxian.)

Draper has assembled this material very well. He has chosen judiciously from the countless casual references and picked significant ones. He has rendered a great service by making available to the English-language reader the hitherto untranslated early drafts by Marx. But he is so blinded by the dogma accepted in the Leninist movement that he sticks to its tired interpretation despite all the evidence to the contrary he himself has gathered. Clearly, the caution and sophistication of Marx and Engels, their ability to view their own past misjudgments in perspective, is lost when ardent followers turn their works into holy writ.

The *Manifesto of the Communist Party* surely needs no new review. It is useful to have it presented, as Dirk J. Struik has done, not only with the introduction written for various editions by Marx and Engels, but also with first drafts, with other companion documents, and with the authors' own accounts of its history. Here the key document is Engels's well-known statement about the history of the Communist League from his *Enthüllungen über den Kommunistenprozess zu Köln,* in which he summarizes how he and Marx got acquainted and began collaborating. As usual, Engels here grossly underrates his own essential contribution, even though he does hint at the facts: namely, that he introduced Marx to the study of the working class and the classics of political economy. When this statement appears side by side with the first drafts of the *Manifesto,* which Engels wrote in the form of catechism, his importance is made even clearer. Indeed, Struik could and should have added several other pieces by Engels, such as the articles he published in *The New Moral World* between November 1843 and May 1845; the lectures he gave at Elberfeld in February of 1845; or the *Outlines of a Critique of Political Economy* published in 1844 in the *Deutsch-Französische Jahrbücher.* It would then have become even clearer that Engels may in fact have been the chief author of the *Manifesto.* But such a conclusion might be painful to a Marxist.

The introduction Struik has provided for this useful volume has virtues and faults. First, his Marxist method has led him to write a splendid survey of the historical background; but the same ideological commitment also makes his discussion of Hegel arid and abstract. Indeed, it is such mumbo-jumbo that I dare any reader, even the most intelligent, to summarize it in his own words. Yet even though Struik clearly is committed to Marxism, he is obviously not a specialist in it; and this lack of expertise shows in a host of subtle ways, any one or few of them too petty to pick out. Further, an uncritical acceptance of the *Manifesto* itself leads him to exaggerate the importance of those persons and groups described and criticized in section III of the *Manifesto.*

Finally, like many adherents, he diminishes the greatness of Marx and Engels by overpraising them or trying to explain away some of their many and inevitable mistakes in judgment. Marx and Engels have always embarrassed their followers who, when encountering statements not in line with orthodox interpretations of the ideology, are forced to make lame explanations or, worse yet, allow themsleves to praise Marx and Engels for their inconsistencies by calling them flexible or able to learn. That always makes the follower look a bit silly. He looks silly, too, when he takes some random remark of no great weight made by one of the two founding fathers and praises it for subtlety or profundity, whereas had someone else made it, he would recognize it as shallow and inconsequential.

Without their introductions, both of these books under review are valuable additions to any Marxologist's shelves.

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The Design of Discord. By Elwin H. Powell. (New York: Oxford University Press, 1970. Pp. iii, 247. $7.50.)

The Design of Discord is a collection of the author's essays that seeks to "deepen and amplify" the meaning of the concept of anomie. This is done, quite successfully and quite indirectly, by focusing on the manifestations of anomie that occur at the individual, societal, and international level. At the individual level, anomie is displayed most dramatically in the act of suicide, and hence successive essays focus on Durkheim's work on suicide; on the relationship between occupation and suicide; on suicide notes left behind in Tulsa, Oklahoma; and on the relationship between race and suicide. At the societal level, successive essays locate anomie in the nature of cities in general and of Buffalo, New York, in particular (between the years 1810–1910); in the incidence of class conflict in Buffalo (1910–1920); and in the incidence of crime in Buffalo (1830–1970). At the international level, anomie is related to the nature of war; the relationship between internal strife and foreign conflict in Europe (1848–1918); the collective suicide of World War I; the "suicidal war cult" of Nazism; and in the integrative and disintegrative forces created by war. All the essays draw together large amounts of previously published material, some rely heavily on new data, and most have not been published previously. In other words, this book does meet its objective: to "deepen and amplify" the meaning of the concept of anomie.

This deepening and amplification, however, is a mixed blessing. For this book succeeds in elaborating the concept of anomie to the point of obfuscation, since the concept is used metaphorically for whatever images are conjured up by acts of violence and situations of conflict. For example, in writing of "the public turmoil of our time," Powell tells us that anomie is "the discord in the rhythm of social life" (p. i); for suicide, anomie is "the loss of purpose" (p. 5) . . . "a general aimlessness accompanied by feelings of emptiness" (p. 8) . . . "the experience of the unstructured" (p. 35) . . . "adolescence . . . : a veritable status-wilderness" (p. 44); for urban conflict, anomie is "the essence of urbanism as a way of life" (p. 59) . . . "the terminal stage in the unfolding of the ethos of individualism" (p. 77) . . . "collective discord" (p. 98) . . . "institutional dislocation" (p. 107); for war, anomie is "general bewilderment" (p. 165) and "nihilism" (p. 173). These metaphoric images may create interesting and even, for some, satisfying insights into the nature of anomie. The criticism of such images is that they are not sufficiently clear to permit systematic inquiry. For this reason, as well as others discussed below, the analytic value of this work is severely limited.

An analysis of the effects of anomie at differing levels of analysis requires that the concept have a constant meaning across each level. With constancy of meaning, the nature of anomie manifested in internal strife can be the same as the nature of anomie manifested in foreign conflict and it is therefore possible to generalize about the effects of one variable—anomie—on different forms of behavior. In this work, however, instead of constancy, we find multiple connotations which constantly shift whenever the term is juxtaposed with a different form of conflictual behavior.

In addition, the effects of anomie on conflict can only be ascertained if "anomie" is something different from the behaviors it affects. In this work, however, this is not the case since "anomie" *is* the conflictual behavior itself. Here, "anomie" does not exist as a discrete variable since it is not viewed, either conceptually or operationally, as independent of the behaviors it purports to explain.

Caveats about the statistical measures also are in order. There are a number of reasons to doubt the unbiased nature of official statistics on suicide, class and racial conflict, and criminal or delinquent acts. For example, it is not reasonable to assume, as this work does, agreement among official standards for classifying such acts, especially among the official standards of different countries. In addition, even if precise standards were shared among different countries, they probably would not be applied uniformly within each country. Those trained to apply precise standards are more likely to be found in urban rather than rural settings: therefore, even without recourse to the concept of anomie, one is likely to find a higher suicide rate in urban settings.

Finally, the analytic value of this work is limited by its insistence on developing a single-factor theory of conflict. As such it either ignores, dismisses, or distorts the meaning of other factors that have been shown or may be shown to have analytic value. For example, in this work anomie is held to have identical effects on both domestic and foreign conflict even though the work of Rummel and others shows these to be independent phenomena.This insistence results in the excessive elaboration of the concept of anomie and in the postulation of a large number of questionable assumptions. In the end, we are left with a dubious metaphor firmly impaled upon Occam's razor.

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Party Strategies in a Multiparty System. By Gunnar Sjöblom. (Lund, Sweden: Student Litteratur, 1968. Pp. 293. 9.70 sk.)

Viewed from the perspective of its scientific aspirations, political science has often been accused of being more scientific in those aspirations than in the facts of their consequence. Typical of this comment are alleged failures to design inquiries that are replicable by other investigators and to integrate prior theory and evidence. This charge is commonly leveled against the social sciences as a whole, a product of a tension that exists between desires to broaden and expand the overall range of theoretical competence (the push for "general theory") and the desire to produce sound "knowable" social facts (the push for detailed, reliable, "behavioral" knowledge.) The social realities we seek to investigate are so complex as to render us impatient with the requirements for detailed and narrowly focused empirical work. At the same time the compulsion to expand the overall level of theoretical competence over social phenomena leads to a proliferation of "theories" which often run far in the lead of the empirical evidence available for any reasonable test of that theory. However one states the question, it would seem that theoretical integration stands as a major need in political analysis with as many partial approaches available as there are personal analytic needs to develop them.

In this volume, Professor Sjöblom has attempted an extensive, if a deliberately noncomprehensive, integration of existing theory relevant to the operation of political parties in multiparty systems. Heavily indebted to a general systems approach, Sjöblom organizes his work to treat such subjects as the nature of the polity and the status of parties within it, actors in the political arena and the goals which orient their actions. Following Easton he examines the concept "output variables" and the process of output determination. Finally, moving closer to the professed interest in strategies he looks at party relations, and exploiting the notion of arenas, identifies three, classed as internal, electoral and parliamentary. In its format the book is a prize of careful organization. Disparate works on parties, electoral behavior, organizational theory, general systems theory, social action theory, and micro-economics are skillfully surveyed and brought into something approaching felicitous union.

Unfortunately, however, the book is more promising as form than substance. The introductory chapter, "Some Problems of Political Science Theory Formation," the best of the eleven, provides ample evidence that Sjöblom understands the task before him, is intimate with the many debates, both historical and current, that swirl about the theoretical literature of political analysis, and is rightfully chary about offering theory where it may be inappropriate. What he offers in its stead is a "scheme of analysis" for the study of party strategies.

> What follows thus cannot in any reasonable sense be called a theory: it can be seen, however, as an attempt at systematic preparatory work for a theory (or theories) based on a probably essential aspect of the behavior of parties. The scheme of analysis is intended to show that a large number of activities by the parties can ultimately be linked to some of their basic aims and that therefore on a certain abstraction level it should be possible to find a high degree of regularity in party behavior" (pp. 12–13).

What does in fact follow this statement is certainly preparatory. Most of the middle chapters read exactly like collections of notes for some future endeavor. An area of interest is identified, defined, and discussed from the standpoint of existing literature. Critical comments are almost never directed toward that literature. These chapters take on the appearance of Sjöblom's running gloss on a literature appropriate to a subject of his interest.

Of what likely use is all this to a student in the field? If on the one hand the purpose in reading such a piece is to acquaint oneself with another's perspective of the subject matter, it may prove interesting. If on the other hand, one is looking for something genuinely integrative, or something "hard" enough to generate usable theory, the book will prove a disappointment. The book is totally non-evidential. The real world is not Sjoblom's concern. The abstractions of his framework do, however, concern him and within it he offers one vague assertion after another, rarely pausing to inquire whether these assertions are linked by a common level of generality. Declaratory assertions of behavioral predisposition are continually offered as if they were empirical propositions, but are revealed upon closer examination to be nothing of the kind. They are merely assertions that in situations a, b, c, . . . x, the actors in a given situation *will* do this or that. Within the context of an explicit theoretical framework such statements might be regarded as tentative propositions susceptible to eventual testing. In the form in which they are presented they are largely empty of meaning. As a whole the work takes on this cast. It is largely an exercise in empty classification.

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Economic and Political Peace. By Shirley Telford. (Portland, Oregon: William and Richards, 1969. Pp. xiv, 224. $5.95.)

This is a remarkable book. It is at the same time a political treatise, a work of learning, and a hoax. I am not sure if it is not meant as a parody; the tone is dead serious but this may be part of the game. It is definitely a Marxist tract; all its scholarly quotations are from two authors, Marx and Engels. These are the only two names in the bibliography. Yet no Marxist of any denomination will probably agree to it.

Mrs. Telford, who professes to be a disciple of Eduard Heimann of the New School for Social Research, claims to have found a new interpretation of Marx's writings. Her message is simple: the new society that Marx had in mind centers around "associated production by competitive producers" (p. 1). It combines free markets with workers' ownership and self-government of the producers. This system "should come into existence by popular vote, not by a forceful overthrow of the capitalist-class" (p. 17).

This preference for a socioeconomic organization which has a large sector of workers' cooperation (similar to that of Yugoslavia) is of course not uncommon, at least among European Socialists. The unusual element in Mrs. Telford's argument is that she does not try to prove the superiority of this arrangement by reference to its merits, or by reference to known historical facts; her whole thesis rests upon extensive quotations from *Capital, Theories on Surplus Value, The Communist Manifesto* and so forth. This makes for odd reading.

The author rejects state communism, capitalism and national socialism and here again Marx and Engles are invoked, page after page. Even strict Marxists will be astonished by this slavish adherence to doctrine; they might want to throw in an occasional dictum from Lenin, Stalin, Mao or other Great Masters. Naturally, most Marxists will also object to Mrs. Telford's idea that the new society will come into being without any kind of revolution.

In her "Introduction" the author states that the Russian reforms of 1965 are said to follow the theories of Yevsei Liberman, but she believes this to be unwarranted; according to her, the economic reforms are due to an American publication: *The Confessions of a Girl Economist* (1963) by Shirley Telford. "Even though the Russians and others have failed to acknowledge me as the author of my new interpretation of Marx, at least they acknowledged the importance of the ideas by accepting them" (page x). She hopes to convince Mao, who, she says, still believes in central planning. In fact, in China a good deal of decentralization is going on, and I would not be surprised if Mrs. Telford believed that this is the result of the present book's publication in 1969 and that the *Confessions* may well have triggered the Cultural Revolution.

If Mrs. Telford had written a study on whether Marx was in favor of workers' cooperation or of central planning, her contribution might have been interesting, though it seems to me that even in this more modest setting her conclusion is not quite convincing. But the author's ambitions make the book look like a joke. I sincerely hope that it is meant as such.

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L'Idéologie Libérale. By André Vachet. (Paris: Editions anthropos, n.d. Pp. 567. Price not given.)

"Liberalism," when it was the thought structure of the *rising* bourgeoise, was historically functional—in Marxist terms "progressive." In the works of recent authors, Mises and Hayek for example, it has become dysfunctional, historically and politico-economically. In the interim the word was transformed in the Anglo-American world, especially in the United States. Movements such as the New Deal appropriated it to mean reformism, even—indeed particularly—in the economic sphere. (To an extent this usage had already been en vogue in England in the era of Campbell-Bannerman and Asquith—Gladstone's Liberalism was still pretty close to the authentic one, though in political theory T. H. Green, the later J. S. Mill, and earlier the Benthamites had laid the groundwork for this change.) This transformation of the term "liberalism," especially since it did not remain confined to the popular language of politics, has, I think, been unfortunate. A "mere" matter of terminology, of course. But one which has not facilitated the teaching and studying of modern political thought, where agreement on "received" terms is of no mean importance and where the job of terminological reorientation represents, at best, a sacrifice of valuable time.

Monsieur Vachet's book traces the development of liberal thought in the traditional and authentic sense—bourgeois-capitalist thought. Very appropriately it is subtitled "the individual and his property." Employing C. B. MacPherson's term "possessive individualism" to characterize it, Vachet defines this dominant aspect of liberalism as the "priority of the requirements of the economic process over the expression of individual independence and interpersonal and social relations" (p. 510). Thus man becomes enslaved by the products of his labor and consequently by those social forces that control the means of production. (No intimation that this may occur also under non-"liberal" systems of political economy. But then Vachet is writing about liberalism, not socialism.) Quoting from Marx's *German Ideology* on this point, Vachet takes it for granted that the bourgeoisie presents its own interest as the universal interest of all members of society. (Which dominant class or ruling group does not?) Liberalism as a thought structure is the expression of this claim to universality. It is thus an "ideology"—in the Marxist sense.

M. Vachet does not ignore the emancipatory and civil libertarian aspect of liberalism—its "juristic freedoms," as the late Professor Franz L. Neumann called it, such as equality before the law—which lends a partial truth to this claim to universality, so that liberalism has never been *entirely* "ideological." But he sees this latter aspect as ancillary to, essentially in conflict with (he places much emphasis on the antinomies of liberalism) and dominated by the former. Nevertheless, he closes on the optimistic note obligatory to socialists of every variety: "Liberalism as ideology and as social practice contains a dynamism or mechanism tending to explode its original intentions and to create the conditions that will overcome it" (p. 513). Thus liberal ideology could not but give rise to the various strands of socialist theory. And in the history of mankind, liberalism's successes and failures have caused new social groups to create new forms of power capable of transforming society by virtue of the material abundance made possible by the "liberal" forces of production. Here, even if one is prepared to credit this rather orthodox faith in the possibility of a truly free and "rational" society, to the extent of commenting that the evidence is not yet in, one must add that it has been a long time in coming.

As Henri Lefebvre points out in his preface, this work represents a synthesis of philosophy, history, and sociology. The synthesis is a happy and successful one. The syncretic approach helps in placing the theories within their historical contexts; in emphasizing the contradictions within the system of thought taken as a whole; in tracing the social origins and pointing up the social effects of the ideas discussed. For a broad work of exposition rather than of specialized scholarship, the analyses are not only impressively thoughtful but also exhaustive, the presentation is always lucid and often profound. The argumentation is convincing; the coverage of source materials, the choice of categories, and the structure and organization of the tome are impeccable—though the latter appears, to an American reader, excessively schematized. I think this is what makes parts of the book rather heavy going, even for one to whom the subject matter is not unfamiliar.

Vachet presents his subject via the following categories: (1) the "themes" of liberalism: naturalism (i.e., the liberal versions of "nature" and "natural law"), rationalism, individualism; (2) the "theses" of liberalism: liberty, equality, property, public safety. Within this framework. he properly places his main emphasis on the thinkers of the late seventeenth and eighteenth centuries—the Physiocrats, Locke, Adam Smith, the Encyclopedists. But he does not exclude other themes. He traces the origins of the liberal and secular ethic to the declining Middle Ages in the work of Duns Scotus and William of Occam. In his fine discussion of Hobbes he stresses, as too many others have failed to do, the role of Hobbesian "absolutism" in the making of the bourgeois-individualist tradition, and he stresses as well Hobbes's affinities with Locke, which are at least as important as the frequently overstated differences between them.

I would fault the book on only two related points: Though all the terrain Vachet takes us over is relevant, some of it has already been very well charted, so that one feels the volume could have been more compact, particularly as there is a bit too much paraphrasing and quoting from secondary works. For example, the often debated relationship between Calvinism-Puritanism and the capitalist spirit is rather too much belabored by leaning on Tawney (Weber is neglected). Laski's *tour de force, The Rise of*

European Liberalism, also receives more attention as a source than necessary. Other examples could be given.

Even if the work as a whole cannot be called an original contribution to scholarship, parts of it are indeed of that sort, and others excel in their analysis and formulation. I think a skillful translation of this book—not an easy task!—would be a real boon to American students in the field

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New Perspectives on Organization Theory: An Empirical Reconsideration of the Marxian and Classical Analyses. By William A. Zwerman. (Westport, Conn.: Greenwood Publishing Corporation, 1970. Pp. XX, 219. $11.50.)

Marxist Sociology in Action: A Sociological Critique of the Marxist Approach to Industrial Relations. By J. A. Banks. (Harrisburg, Pa.: Stackpole Books, 1970. Pp. 324. $11.50.)

During the last hundred years Marxist theories have appeared in many roles and have been applied for various purposes. Recently some social scientists have tended to translate them into nonideological, purely scientific theoretical models and have hoped to use them as a means to overcome the parochiality and lack of coherence of empirical research; the books of Zwerman and Banks are good examples of this approach, through their methodological procedures are basically different.

Zwerman confronts his empirical findings, assembled without any theoretical presumptions, with Marxist and classical business theory; Banks has taken some of Marx's theoretical theses as his point of departure and has looked for empirical data which might verify or contradict them.

Zwerman's empirical findings are an extension and amplification, on the basis of American data, of the now famous investigations of the English sociologist, Joan Woodward. Professor Woodward had proved that in British industry technological factors determined fundamental organizational features, such as the style of management, shape and form of organization, and character and application of the labor force. Zwerman's research confirmed the validity of almost all of Joan Woodward's findings and are in this respect a tribute to the methodology employed and hypotheses put forward by this outstanding British scholar.

As far as America is concerned, Zwerman is basically right in saying that, with few exceptions, scholars engaged in the analysis of formal organizations have typically played down the pioneering work done by Marx in this field and have almost entirely eliminated Marx's theoretical perspective; conservative business management theory and functional analysis have nearly monopolized formal organization theory. Therefore, Zwerman's research will probably contribute a great deal to the shaping of new theoretical perspectives in American organization theory.

It must be mentioned that in Europe, however, the interest in technological determinants of organizational structures is general among industrial sociologists. Europeans will primarily appreciate in Zwerman's study the introduction of new variables that are highly important in contemporary industry, namely the relationship between ownership and management, the ratio of nonmanagerial supervisors to managerial staff, and the dependence on local markets. All readers will appreciate the exactitude and rigor of Zwerman's empirical approach and the elegance of his presentation.

Banks deals in his book with much wider and more general problems than Zwerman. In his deliberations on Marxist theory he proceeds from the most general assumptions to theses referring to rather specific, middle-range problems. His opinion is that the most general premises of Marxism, such as Marx's theory of social change, were not susceptible to empirical verification. He apparently believes the same was true of Marx's less general theses, such as the theory of capitalism. The theses referring to specific problems are considered by Banks as either a conversion of the processes of social change under capitalism into recognizable relationships between social classes or as the application of the more general premises of the theory of capitalism to specific problems of industrial relations. It is on that lower level that he does regard Marx's theses as verifiable by empirical procedures.

In order to verify Marx's assumptions Banks has used statistical data, results of economic analyses, and historical-descriptive studies; thus his investigations are free from the onesidedness of survey and questionnaire methods. He seems to be aware of the numerous gaps and shortcomings of the available sources. He has been remarkably successful in avoiding this difficulty by relating some points of his analysis to the British iron and steel industry about which an enormous wealth of primary sources, including good monographs, was available.

Banks deals in his book with problems which are either overlooked by many non-Marxist writers on industrial relations or treated by most Marxist writers in a traditional, dogmatic way. Banks's most important contribution in this respect is his analysis of the evolution of working-class political attitudes; it is in this field that he aptly confronted Marx's conclusions with the contemporary situation in the industrialized part of the world. ". . . the historical trend in industrial relations in Britain," he writes (p. 136), "has been not simply towards class-consciousness which will abolish capitalism, but towards what might be called 'organizational consciousness' to create permanent machinery for dealing with grievances in the industrial context."

His analysis, presented with great knowledge and deep sociological insight, leads him to accept some of Marx's views concerning the impact of industrial development on trade-union militancy. Banks rejects the notion that direct action is the only indicator of militancy; he thinks that the deepening class consciousness expressed in growing opposition to complacency and compromise is of no lesser importance.

Dealing with other problems, such as the factors shaping working-class consciousness, the patterns of trade union activity, or the character of leadership, Banks concludes that Marx's analysis, although not falsified by recent developments, needs much amplification and clarification. Moreover, in some fundamental respects Banks completely rejects Marx's assumptions. In his view, the change in the character of industrial relations was the result of the transition from capitalism to some sort of voluntary collectivism, i.e., a new type of society with different economic foundations and a new class and power structure. "Private enterprise capitalism, so far from being converted into state, municipal and cooperative enterprise has been replaced by private enterprise collectivism involving welfare state politics on a pluralistic basis" (p. 198). New functions of trade unionism, collective bargaining, programs of extending state control over industry, possibilities of industrial democracy—all these issues are discussed by Banks in the context of voluntary collectivism.

But in view of these deliberations and conclusions the problem of verifying Marx's long-term prognoses seems to be insoluble. Marx's predictions became a "self-destroying prophecy" of sorts. As far as they proved true at the turn of the nineteenth and twentieth centuries, they became codeterminants of fundamental social changes which transformed the whole fabric of industrial societies and made many of Marx's concepts unapplicable to the new social order.

Banks argues that "collectivism" is a new type of class society in Marxist terms, but it is difficult to believe that what he calls "collectivist exploitation of surplus labor" in organizational societies may be analyzed by making use of Marx's theses on free enterprise capitalism. This means in turn that the assessment of "the possible contribution of Marxist thinking to the development of truly scientific theory of sociology" (p. 18) cannot be founded on middle range theories, that is, on Marx's views on particular and transitional structures of free competitive capitalism as Marx knew it. What seems to be particularly relevant in Marxism nowadays are not so much the answers and solutions offered by Marx, but the questions that he asked, the factors and variables he regarded as essential, and the analytical procedures he used. It is exactly this general aspect of Banks's interest in Marxism that makes his study so valuable to students of industrial relations.

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Ombudsman Papers: American Experience and Proposals. By Stanley V. Anderson. (University of California, Berkeley: Institute of Governmental Studies, 1969. pp. 407. $3.75.)

If we are indeed "on the threshold of an Ombudsman explosion," as Professor Anderson wrote in 1969 in his Introduction to his *Ombudsman Papers: American Experience and Proposals* (p. 1), it is in no small measure due to his enthusiasm, perseverance, salesmanship, and productivity. For a dozen years or so he has been one of the institution's most impressive and consistent advocates, and he was one of that very small group of American advocates—among them Kenneth Culp Davis, Brian Chapman, Congressman Henry S. Reuss (D. Wisc.), and myself—who wrote in the late 1950s and early 1960s to try to convince America of the ombudsman's virtues long before Walter Gellhorn's well-supported and publicized works began to appear in print.

It is thus hardly astonishing that this Anderson tome—published in a somewhat odd format and type by the Institute of Governmental Studies, is—as the author frankly acknowledges—"a plea for adapting the ombudsman idea to American institutions, a plea based on the conviction that ombudsmen are humanizers" (p. 72). I wholeheartedly support his conviction as well as his plea as it applies to state and local levels as well as to such non or quasigovernmental sectors as the campus, corporations, and consumers. But it is especially at the bureaucratic level that the ombudsman's role is at once most needed and most promising, for given the proper climate and tools, he (or she —*bien entendu*!) can most effectively address himself to society's grievances as these are presented to him in form of complaints.

Yet, as Anderson wisely points out, unrealistic expectations must be eschewed. Governmental ombudsmen cannot create better transportation systems; build low-cost housing; eradicate unemployment; or contain "long hot summer violence." Campus ombudsmen cannot prevent the physical seizure of buildings (e.g., C.C.N.Y.); but they can—and do, as Anderson documents—serve effectively as people's watchdogs for the publicity and redress of grievances (e.g., Nassau County); for the rectification of procedural (and sometimes substantive) inequities, aberrations, and injustices (e.g., Hawaii); as fertile sources of positive, non-axe-grinding recommendations (e.g., Buffalo); and as important tone setters and morale builders (e.g., Denmark).

Of crucial importance to the institution's success, however, is not only the community's unstinting support but also the identity and personality of the ombudsman. Professor Stephan Hurwitz's unqualified, total acceptance and success as Denmark's ombudsman for fifteen years —not one of his recommendations or findings was rejected by the agency or individual concerned—was due to his consummate ability, tact, poise, sagacity, perceptiveness, and diplomatic *savoir faire*. Moreover, his unanimous selection by Parliament had been widely applauded by the public. It is difficult, of course, to emulate that example, yet we, too, ought to be capable of broadly accepted choices—we shall I hope, soon once again resolve to reduce the contemporary emphasis on a veritable Noah's Ark concept of representativeness! Without support and confidence the ombudsman cannot function in America or anywhere else; nor can he function without at least a willingness to understand the purpose and operation of the institution.

Professor Anderson has compiled a valuable collection of analytical and expository materials on both the legislative history of the ombudsman and its *modus operandi* on the American scene. He defines the institution's essential characteristics and its application; he evaluates and traces sundry proposals and implementations; and he reports specifically on its operation at state, local, and campus levels. A sensible conclusion is followed by several helpful charts, tables, and appendices, augmented by a superb (chart-format) comparative analysis of ten civil ombudsmen offices by Professor Kent M. Weeks of the College of Wooster.

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The Search for an American Indian Identity: Modern Pan-Indian Movements. By Hazel W. Hertzberg. (Syracuse, N.Y.: Syracuse University Press, 1971. Pp. ix, 362. $12.00.)

American Indians and Federal Aid. By Alan L. Sorkin. (Washington, D.C.: Brookings Instituition, 1971. Pp. viii, 231. $7.50.)

The two books under review differ in both subject matter and approach. They are united only in their significance and utility to the student eager to understand the situation of the American Indian today. Sorkin's book, a typical Brookings Institution study, will probably be of greater interest to most readers of the *American Political Science Review* than Hertzberg's book, although the latter in many ways has more permanent value. Sorkin's volume, the eighth of the Brookings Studies in Social Economics, describes and evaluates the various programs of assistance to the American Indian carried out by the Federal Government. These programs fall in the areas of education, health, agricultural development, industrial development, manpower development, property and income management, and welfare services. Each category is dealt with in a separate chapter supported by numerous statistical tables. Data are frequently drawn from unpublished sources and from interviews with officials dealt with by Mr. Sorkin in the course of his research. A preliminary chapter provides a general look at the reservation Indian (the subject of Bureau of Indian Affairs concern), both historically and statistically, in terms of population, income, unemployment, education, and the like. A final chapter suggests "A Program for Improvement."

In the Brookings tradition, Sorkin does not merely describe but also evaluates the effect of the programs he discusses, although his evaluation is invariably cautious and qualified by statements of the "on the one hand . . . on the other hand" type. Often his judgments are questionable. In discussing the Indian Resources Development Act, rejected by Congress in 1967, for example, Sorkin considers "unfounded" Indian criticism of the provision to allow the Secretary of the Interior to approve or disapprove loans over $60,000 and the use of proceeds from trust property sold, and

Indian criticism of the provision to allow tribal land to be mortgaged to obtain project financing. In regard to the first point, Sorkin asserts that "it is unlikely that this power would have been used restrictively," and "the secretary has been quite flexible toward the investment and disposal of Indian trust funds and tribal income totaling several hundred million dollars" (p. 98). In regard to the second point, Sorkin asserts that "it would appear that the advantages of being able to mortgage Indian lands would have outweighed the disadvantages," since the ability of the tribes to borrow funds would have been enhanced and "some tribes whose sole asset is land would have been able to participate in the lendable funds market" (p. 98). Sorkin makes only a perfunctory "on the other hand" concession that while the Indian objections "appear unfounded, it may be true, as some opponents charged, that the bill would have been a prelude to termination" (p. 99). Indeed it might, and Sorkin's sensitivity to Indian cultural and psychological needs is here demonstrably weaker than his perception of an Indian future cast in a white economic mold.

Sorkin's recommendations are best when they relate to details of existing programs already accepted by Indian people. His recommendations aside, which one can take or leave, his book is a compendium of up-to-date information that is indispensable for the scholar or policy maker concerned with the present economic status of the American Indian.

The Hertzberg book provides the only detailed study of modern pan-Indian movements. The author's emphasis is on the first thirty-five years of the twentieth century, but she carries the story rapidly through the second thirty years. One might talk about the rise and fall of such movements, since many have fallen victim to the bitter factionalism characteristic of Indian movements and indeed, as Dr. Hertzberg points out, of all reform movements. Hertzberg divides pan-Indian movements into three groups: reform, fraternal, and religious. The first half of the book deals with the Society of American Indians, the most important reform pan-Indian group of the twentieth century. The Society, organized in 1911 in Columbus, Ohio, under the leadership of a white sociologist, F. A. McKenzie, dissolved in factional disputes ten years later, after compiling a significant record of accomplishment in publications, lobbying, education and the like. Although organized at the suggestion of a white, the Society excluded whites from full membership and participation in its activities. In the Winter, 1970 issue of *The Indian Historian*, published by a newly arisen pan-Indian organization, the American Indian Historical Society, I wrote a brief description of the Society and called upon an Indian to tell the fascinating account of its rise and fall. I wrote the article because I discovered that no one I asked—including distinguished scholars, Indian and white, and officials of contemporary pan-Indian organizations—knew anything about the Society or its important journal. Fortunately or unfortunately, my call proved unnecessary, because Professor Hertzberg, of Teachers College, Columbia (although not an Indian), has amply documented the history of the Society in this book. I can confirm her statement that "the history of Pan-Indianism is almost completely unknown even to contemporary Pan-Indians, despite the fact that modern Pan-Indianism has existed for two-thirds of a century" (p. 297). I hope her book will make this statement obsolete.

Prof. Hertzberg recounts the conflict between two leaders of the Society. One was Arthur C. Parker (son of a Seneca father and a white mother), an anthropologist who became editor of the SAI's magazine, its secretary, and eventually its president. The other was Carlos Montezuma, an Apache who as a child was captured by the Pimas, was sold to a white man, and later became a doctor. The story of their conflict provides a textbook example of how internecine warfare can develop from the best intentions. Montezuma, an activist, had a simple and forthright program: Abolish the Indian Bureau and "free" the Indian from its control. After several years of skirmishing with Parker and the leadership of the SAI, which sought to influence or capture rather than destroy the sources of power over the Indian, Dr. Montezuma resigned and started his own journal which he pledged to publish until the Bureau of Indian Affairs was abolished. Needless to say, the BIA has outlasted Montezuma's publication, *Wassaja*. The Parker-Montezuma conflict is only one of many stresses that eventually destroyed the society, all recounted at length.

Prof. Hertzberg's book is based primarily on manuscript sources, most notably the Arthur C. Parker papers at the New York State Museum in Albany. (An inexplicable omission is her apparent failure to consult the Fayette Avery McKenzie Collection at the Tennessee State Library and Archives in Nashville.) The author expresses the hope that her book will spur other scholars to discover other collections of pan-Indian material, particularly concerning the fraternal—and primarily urban—pan-Indian groups. She herself deals only briefly with fraternal pan-Indianism, both because of the scarcity of materials and because her study covers only movements national in scope. Fraternal pan-Indianism often was local in character, lacking national organization. Hertzberg is similarly brief in her discussion of the peyote cult and religious pan-Indianism, because several good studies of the movement, as eventually organized in the Native American Church, already exist.

Although the author's prose is lumbering and often diffuse, her analysis is keen and perceptive. It has, nevertheless, to be dug out of the material she presents. Thus both Sorkin and Hertzberg have given us valuable books, although we need not accept either their style or their frames of reference.

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Congress: Two Decades of Analysis. By Ralph K. Huitt and Robert L. Peabody. (New York: Harper & Row, 1969. Pp. 241. $3.95.)

This volume includes a review of research on Congress by Robert Peabody and six previously published studies of Congress by Ralph Huitt. To judge the volume is primarily to assess Huitt's major work and secondarily to assess the present state of congressional research.

The success of a political scientist, according to Peabody, is judged by his professional peers and by his contributions to the growth of a body of knowledge and a discipline. By these standards, there is no doubt of Huitt's success. Students of Congress are indebted to him; he has strongly influenced the manner in which they perceive the institution. Political science is indebted to him; by artfully combining theory, common sense, and careful observation, he has helped to shape the discipline. Huitt's success is based largely on his intuitive grasp of men and their institutions as well as his ability to concentrate on the significant, despite the lure of the periphery. These talents are personal, not methodological. They are strikingly revealed in all his work—whether drawing on documentary sources ("The Congressional Committee" and "The Morse Committee Assignment Controversy") or relying mainly on participant observation ("Democratic Party Leadership in the Senate" and "The Outsider in the Senate") or ranging broadly and speculatively ("The Internal Distribution of Influence: The Senate" and "Congress, the Durable Partner").

Talents such as Huitt's are not randomly directed; they are guided by concepts derived from role theory. The roles of members of a committee, the role of the Democratic leader, the norms of the Senate, and the role of the "outsider" are all explored in depth. Role concepts for Huitt are orienting, as they seem also be be for Fenno; they are not binding or limiting, as they appear to be in studies by Wahlke and Eulau and by Davidson. There is nothing as categorical, as reassuring as are "delegates" and "trustees," "tribunes" and "ritualists." Instead, roles are extremely varied and often conflicting. Nor is there a simple explanation of why certain roles are adopted and not others. As far as the committee member is concerned, party, constituency, interest group, and occupational affiliations all count. As far as the outsider in the Senate is concerned, the role Senator Proxmire chose was one suited to his personality and temperament, one he had played before, one in line with his goals, one congenial to his independent Wisconsin constituency, and one recognized as legitimate, if not popular, by his colleagues.

Huitt regards the concept of role as a means, but not the end, of inquiry. By itself, the concept falls short. A senator's interpretation of information, of facts is shaped by his preconceptions, allegiances, and perceptions. This is shown in the study of the congressional committee. But a senator's behavior is also shaped by situational factors. This is shown in the studies of leadership and committee assignments. Leadership behavior depends not only on the leader's subjective view of his relationship to the presidency, but also on his objective situation vis-à-vis the White House and members of the Senate. Which party controls the Executive, which the Congress; what policies are at stake; and how are men, preferences, and resources distributed. Much depends on Senate norms, but norms rarely require particular behaviors. They allow competing claims and permit alternative actions; and outcomes vary according to how situations are structured, what leaders and members do to restructure them, and just how they go about their tasks.

The purpose of Huitt's case studies of individual senators, including their roles and the situations in which they find themselves, is to increase his own and our understanding of Congress. Some researchers study congressmen, some analyze votes, and others fasten on roles—abstracting and simplifying as is necessary. On occasion, however, they become so absorbed by the elements that they disregard the context. Huitt is different. He never loses sight of Congress as an institution, an institution that is something more than the sum of members, behaviors, or constructs. To him Congress is complex, and he is acutely aware that "some familiarity with senators and their world brings with it a hesitancy to say very much with certainty." Certain or not, he says much that makes sense—about the institution, how it works, and why it does.

Ralph Huitt may be pre-eminent, but he is not alone. Peabody's incisive review of research from the mid-1940s to 1967 describes a number of important contributions by students of Congress. It should be noted that some of them owe much specifically to both Huitt and Peabody, who headed the American Political Science Association's Study of Congress project, begun in early 1964. Empirical research on Congress is in a healthy state. Yet no one—and not Huitt—has produced a "conceptually clear and comprehensive theory." Peabody would like a scientific breakthrough. So would most of us. In this respect, there is little reason to be optimistic. No overarching theory is on the horizon, and surely none that is likely to do a job on Congress. Yet there is little cause for distress. Much has been accomplished in the recent past, and the payoffs have been rich. New endeavors, such as the examination of "policy systems" proposed by Huitt, promise even more. I for one can curb my appetite and happily settle for the valuable fragments of knowledge served up by Huitt and other sensitive students of Congress.

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The Basic Symbols of the American Political Tradition. By Willmoore Kendall and George W. Carey. (Baton Rouge: Louisiana State University Press, 1970. Pp. 163. $6.00.)

Ambitiously, the authors aim to reinterpret the whole American political tradition. Central to this revaluation is the proposition that Lincoln's interpretation of the Declaration of Independence is heresy (pp. 88–94). More specifically, the interpretation of "the 'all men are created equal' clause" in the years since Lincoln has caused "a derailment in our tradition" (p. 84). A century-old "derailment"—wreck—of the train of our oldest beliefs is a serious matter. Because wrecked beliefs cause wrecked policies, the authors are not content to criticize the now dominant equalitarianism in our tradition. They also wish to reappreciate the older democratic republicanism of pre–Civil War and pre–Revolutionary America. Therefore, this book is radical: it brings to light the roots of our tradition in order to make questionable their present-day fruits.

Two events justifying this questionable enterprise are mentioned. First is the crisis of American self-understanding caused by those Lincolnians, the progressivist-revisionist historians. The version of the past first elaborated by Beard and carried further by Hofstadter, Dahl, Burns (not to mention Randall), and others teaches that all political opinions are equal or that Americanism is holding that no political opinion is un-American (pp. 5–7). Because the great American democrats did hold strong opinions about what is un-American, say the authors, these historians are out of sympathy with our past. While claiming commitment to democratic "values," progressivist-revisionist historiography in fact corrupts American democracy by not appreciating the great speeches and deeds of our political heroes. Thus the authors desire to ask again, "What is traditional amongst us?" What is oldest and best for us? The recent development of Voegelinian "vocabulary and intellectual apparatus," the second event making possible this enterprise, provides the answer to these questions. Voegelin's conception of tradition as a people's means to organize itself for "action in history" by continuously representing itself to itself through a process of symbolic differentiation inspires our authors to revalue the American political tradition.

Following Voegelin's "practice," our authoritative "symbol" is discovered: rule by the deliberative sense of the community of virtuous men, men who understand themselves to be under sacred restraints. This symbol is already present in a "compressed" or "undifferentiated" form in our earliest public document, the Mayflower Compact. All major public documents through the Bill of Rights make this symbol more meaningful, differentiate it into peculiarly American institutional, political, economic, and socioreligious symbolic forms. Some time after 1789 and before 1860, derailment occurred. Americans became primarily dedicated to "equality," to "pursuing policies designed to make 'all men equal' socially, economically, and politically." Only secondarily do they cling to "self-government through deliberative processes," e.g., to legislative supremacy. Because of Lincoln's speeches and deeds, there have been "two traditions" which are "not compatible with one another" (pp. 94–5). Kendall and Carey, it seems, hope for one harmonious tradition; they hope to "derail" that tradition "which holds to a rather extreme view of equalilty."

Why then do they almost ignore the large and growing scholarly criticism of progressivist-revisionist historiography? Such an oversight seems puzzling because the antirevisionist historiography—for example, the writings of Adair, Stourzh, Elkins and McKitrick, Diamond, Jaffa, Wood, Kenyon, Bailyn, and later Hofstadter—largely discredits the view that we have two antagonistic traditions and so produces a more harmonious view of our tradition. Why do the authors in effect accept the most fundamental presupposition of the progressivists, the opinion taught by Hofstadter to so many that there is an "antagonism . . . between the philosophy of the Constitution and the philosophy of American democracy"? Why do they only argue that—to use Hofstadter's terms again—"the philosophy of the Constitution" is morally and politically superior to "the philosophy of American democracy"? Perhaps their acceptance is for argument's sake. After all, the progressivist version of the tradition is still that most often taught in high schools and colleges. But does not our great tradition enjoin us to accept not its current or dominant but its true version?

Not the lowly demands of rhetoric alone, I suggest, but the authors' or their authority's conception of tradition necessitates the acceptance of the now traditional understanding of the tradition. According to this conception, traditions result from a processive differentiation of the compressed symbols of a natively unique experience. Compressed symbols cannot appear as such to those who undergo the originary experience; the Mayflower voyagers could know nothing of the differentiation of compressed symbols. First symbols appear "compressed, by comparison with that which they will become with the passing of time" (p. 24). What compressed symbols become is "unpredictable." Indeed, such symbols are capable of "an infinite variety of potential developments" (p. 37). Political philosophy now seems to know about such symbols and what happened to them in time. But political philosophy, we discover, is itself a differentiation of symbols, a "critical clarification" of traditional symbols that uses "the symbols as its raw material" (p. 25). So this Voegelinian interpretation of our political tradition requires that the traditional or publicly dominant symbolization of the tradition be received as the object of critical clarification. Since that symbolization is understood as essentially accidental, this interpretation depends upon the "passing of time." Its truth is temporal and, therefore, not truth in the traditional or philosophic sense. But perhaps Voegelin's representation of symbolization will be shown to be the compressed symbol of the absolute experience. Until it is, *Basic Symbols* will stand as an attempt to criticize the times from the point of view of the times.

In spite of or because of this root difficulty, the book has pedagogic value. To accomplish their questionable aim the authors must state the dominant view of the tradition and its characteristic presuppositions. This they do with clarity and force. With equal force, they show this view's theoretical and historical difficulties. If they have not succeeded in understanding our political experience better than our great political heroes did, they certainly understand it better than the now dominant scholarship does.

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Power and Community: Dissenting Essays in Political Science. Edited by Philip Green and Sanford Levinson. (New York: Pantheon Books, 1970. Pp. ix, 396. $10.00, $2.75 paper.)

This collection of essays is an outgrowth of the Caucus for a New Political Science. It more or less accurately reflects that group's unhappiness with so-called mainstream political science. In a lead-off essay, Euben ascribes many of the problems of the discipline to the trauma of Nazism and McCarthy. One who remembers the way in which populist became a pejorative term can find some merit in the history. Not only did the leaders of the discipline develop a fear of the masses and old fashioned democracy, they began to rationalize a pluralistic group competition with an apathetic citizenry as a reasonable facsimile of all that democracy really need be. Political science had become part of the establishment and unconsciously began to pay its way by rationalizing the status quo in true Panglossian style. Beyond the discipline's love affair with the Land of the Free, it suffered a further astigmatism by one-sided attachment to the national cross-sectional sample survey, the endless and empty though honorific voting studies, and mindless statistical exercises in correlation analysis.

Were this the whole of Euben's complaint, many would agree with him. However, he embarks on an application of Kuhn's Structure of Scientific Revolutions to the discipline in apparent innocence of Kuhn's subsequent abandonment of the position taken in that book and Stephen Toulmin's powerful criticism of it. Following Kuhn, he comes precious close to making political science a mere procession of momentary fads without independent empirical warrant, corrigibility, or possibility of cumulative improvement. This may be right, but one must hope, if the discipline is to have value as other than rhetoric, that there is a better possibility.

Levinson, I would like to believe, is disturbed by Euben's extreme posture especially since he seems to be honestly and honorably concerned with whether or not he has something of value to teach his students about useful modes of explanation and reasoned evaluation. His own experience at the feet of Kissinger must render him chary not only of the mandarin but of others whose warrant to teach is even less susceptible of rational criticism. But he is surely right that asking how the discipline might provide its students with really useful tools would be refreshing in its impact.

Dolbeare makes the point, with which more and more are coming to agree, that the discipline has been too wrapped up in institutions and institutional inputs to the almost complete neglect of outputs, particularly as these are observable in the lives of the underlying population. "Who Governs?" needs to be supplemented by the question what difference does it make to whom who governs and the mode of their governing. The burden of many of the essays is that a favorable verdict has been bestowed without inquiry.

Rogin attacks what he claims to be Truman's failure to deal with the political process of groups in the same manner as with states. National interest is treated as a "spook" but group interest is treated in a different fashion. Rogin sees the conflict between leaders and rank and file in labor as an important reason why interest group liberalism fails to yield the democratic fruits claimed for it. One may wonder whether Rogin thinks Lenin wrong that the working class by itself doesn't rise above trade union consciousness or holds Truman at fault for not making Lenin's point.

Lipsitz finds Lane and Converse to consign "the poor," whatever this term may mean, to a satisfied conservatism or a confused inarticulateness. His *n*, several times that of Lane but still scarcely significant, shows that the "poor" have grievances. Presumably, a truly democratic system would achieve their articulation. Moynihan, if he had taken off his technician's blinders, might have had more tolerance of the O.E.O. experiment.

Parenti finds the classical image of ethnic integration misleading. Irish, Poles, Italians, and others have been invited to assume American burdens but not join the country club. The condition of Negroes is far worse. From the most rabidly assimilationist of the ethnics, they have been forced to the perspectives of black nationalism on the one hand and class struggle on the other. While Parenti sympathizes with the class struggle position of the Panthers, he finds value in the nationalist posture as well. He sees the Negro future as bleak, depending on a social revolution that only whites can make. It would be interesting to examine what data and what numbers make this perspective persuasive.

Wolfe recounts his experience of the attempt to create a community, with the creation of community as the purpose at Old Westbury. Not surprisingly he finds this was the wrong way round. You have to have purposes in common that make a community a useful enterprise.

Walzer raises one of the most engaging ideas in the volume. Taking off from the great sitdown strikes of the automobile industry and implicitly raising questions of university governance, he examines the possibility of the civil disobedience of corporations without utter incivility or a confrontation with the state. This essay has the great merit of being grounded in history and addressing concrete continuing problems. One would like to see it carried further.

Green discusses the revolutionary implications of Ocean Hill-Brownsville but mainly in terms of a polemic with Mayer. He assumes that the possession of a school board creates a black community in the district. Would that that would do it. Those blacks and whites trying to develop leadership and organization in the ghetto would have a ready made nostrum. It would also have helped if Green had been able to tell in what other than an intuitive way he measures the good done the people of the district and the children of Ocean Hill-Brownsville. These need more than taking on faith.

Schaar, speaking in a voice oddly reminiscent of William Y. Elliott attests the decline of authority destroyed by the workings of a rational-legal order and the abundance of the bored middle class. There is a sense in which we all would like the poetry of religion and tradition like the mullioned windows of a Gothic Cathedral, to darkly illuminate what reason will not warrant. James the First attacked those who would wade into the mystery of kings. But as Schaar himself with Weber recognizes we can't go back. Nor is the case except for the bored unemployed academic so hopeless. Human misery and vast scarcities abound. The bored vapid vanity of affluence is the occupational disease of a well paid academia and its middle-class pupils.

Tracy Strong writes an essay "Hold On to Your Brains." It's a hard ride. He cites with approval Nietzsche. "As I understand Nietzsche, he is writing in such a manner that everyone who reads him is forced or drawn to read *himself* into what Nietzsche wrote." This new epistemology is apparently unburdened by the law of the excluded middle and offers limitless possibilities for a new political science.

McWilliams winds up the volume with an essay of which only a very fastidious Platonist would disapprove. On balance, with exceptions, these essays are not so much an attack on mainstream political science as a plea for a return to an older genre of the discipline, belletristic political theorizing in the mode of the *New York Review of Books.* It is difficult to answer the vulgar question who would do what differently and better as a result of reading many but not all of these essays. Somewhere Marx writes, up to now the philosophers have only interpreted reality; the important thing is to change it. These are for the most part philosophers. What and how and if they seriously want to change remains obscure.

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Toward a New Public Administration: The Minnowbrook Perspective. Frank Marini, ed. (Scranton: Chandler Publishing Company, 1971. Pp. 372. No price listed.)

Dwight Waldo, feeling that the times were out of joint, and startled by a young professor's assertion that a special issue of the Public Administration Review constituted "old men talking to old men about irrelevancies," took the initiative in organizing a conference of "young Public Administrationists" at Syracuse University's retreat at Minnowbrook in New York in September, 1968. The conferees were "to discuss whatever seemed important to them." The papers and commentary were assembled and printed in book form, *Toward a New Public Administration,* subtitled *The Minnowbrook Perspective.* Frank Marini of Syracuse edited the book.

There are twelve papers, most followed by comments, often short papers themselves. In addition, short portions of the tapes of the meetings are frequently inserted to further illustrate their tenor. All but one of the twenty-two contributors are academics, most, assistant professors (as of 1970); all are "young," but the criterion of youth is not disclosed. Given a conference so organized, readers will probably expect to meet a predictable syndrome of ideas, assertions, criticisms, and emotions. They will not be disappointed. This syndrome "came to be called the Minnowbrook perspective" (p. xv).

Although the tone of the conference, as revealed in papers, commentary, and edited tapes was unmistakable, not all participants shared the "Perspective" or bought it one-hundred per cent. Many arguments, therefore, were at least easy for me to understand. Since we know that administration, being government, is inevitably political, a more self-conscious recognition of the administrator's values should contribute to responsibility. In a world of rapid change, the administrative objective might well be development—an evolving, adaptive, problem-solving structure rather than classical rationality. Recognizing new needs for change, innovation, and clientele involvement, and recognizing also the inevitable value commitments of administrators, might indeed produce a more realistic public administration theory. We are told that social, technical and cultural changes may require modification of Weberian bureaucracy in the direction of short-term, situationally determined, project organization. An orthodox assessment of the scientific validity of administrative theory is combined in one paper with a criticism of the moral basis of such theory, deemed to be poor "if our assessment is sensitive to the preferences of minority groups or underprivileged participants in a political system" (Kronenberg, p. 215).

A few papers could have been presented comfortably to a Conference of Older Public Administrationists—for example, Henderson's adequate paper on comparative administration and Sharkansky's paper on policy making, which comes the closest to an empirical study in the classical scientific style. And a few people (for example, Howard) even expressed doubts about that prestigeful new governing device—PPB—in line with Wildavsky's warning that government is inescapably political, not economic.

The last three papers are by Syracuse faculty members. Frederickson attempts to foresee the future of a public administration with a "social equity" basis. Lambright tells us that "Public Administration *is* Public-Policy Making"; and Marini, expressing some "Objections to Religious Crusades," attempts to summarize the "Minnowbrook Perspective."

Perhaps more interesting, if not more enlightening, than these digestable proposals is the mixture of notions and emotions which constitute the "Perspective." (Remember, as far as this book is concerned, the "Perspective," alternately the "New Public Administration," is what we, the readers, have been expressly offered.)

A dominant theme is the search for "relevance," a term never really defined. Generally accepted is the notion that this is a period of revolutionary ferment and change, but no indices of scope or rate of change are produced. There is even an occasional reference to this "repressive" society. ("Perhaps more attention should be devoted to exploring the options available to administrators in a society bent on repression" [Friedland, p. 54]). In such a conference one is not surprised to find commitment to "the Politics of Love" (White, p. 69), defined as consensus behind absolute ethical norms and a political process of confrontation rather than negotiation and compromise by representatives. Apparently, history is or ought to be a progression from status to contract to confrontation. However, the exaggerated claims for youth (". . . the modern young person is undeniably more mature than his counterpart of even a generation ago" (White, p. 61) might indicate a movement from contract (achievement) back to status.

Many of the contributors accept the notion that the American system is coming apart. "It has now become painfully visible to our society that it is the owner of decaying cities, racial inequities, civil strife, . . . inadequacies in the quality and responsiveness of a large array of governmental services, and so on" (Biller, p. 96). Dogmatic terms like "painfully obvious" abound, and one suspects that, as usual, they indicate a paucity of data.

The contributors, for the most part, reject the notion of an objective social science. Knowledge is subjective, value-laden, and culture-bound, a result of social brainwashing supporting the *status quo*. Since it must always project value preferences, they urge that social science, and especially public administration, adopt an explicit value orientation. Roughly, this orientation should be the promotion of equity in income and power—a strong identification with the interests of fairly powerless minorities. To bolster this position, they reject "positivism" and hopefully call upon phenomenology and existentialism. The structure of consciousness itself qualifies the purity of our knowledge. Hence, self-analysis, self-revelation, empathy are all parts of knowledge gathering. One must "expand consciousness." (See especially the paper by Kirkhart and following commentary.)

These views lead to an attack on democracy as we experience it. ". . . the procedures of representative democracy presently operate in a way that either fails or only very gradually attempts to reverse systematic discrimination against disadvantaged minorities" (Frederickson, p. 311). This criticism also applies to public administration, which is too majoritarian. It does not reflect "the preferences of minority groups or underprivileged participants in a political system" (Kronenberg, p. 215).

Consequently, "the new Public Administration should be concerned with making the public bureaucracy an instrument for achieving social justice and equality. This concern is very different from that of simply making the government responsive to the wishes of a majority of the people" (Zimring, p. 231). Public administration should be oriented toward the clientele, provided they are underprivileged, not the popular sovereign whose servants we thought administrators to be. There is too *much* distinction between policy and administration. Administrators should be political.

But how can public administration thumb its nose at majorities, or at the actual holders of power? Why should the establishment support such an administration? How do you transfer power from those who have it to those who do not? They have no solutions and barely discuss this problem. The only suggestion is an ominous one. ". . . it may be necessary for the new Public Administration to develop outside of the existing institutional framework and thinking of the university and government" (Zimring, p. 233).

This group was little concerned with techniques to improve administration. Why increase the effectiveness of a bad institution? For many of them, however, PPB is different. It is related to the concern for social justice. Perhaps this method will work where pluralistic democracy, with its incrementalism, failed. "PPB can be used as a basic device for change . . ." (Frederickson, p. 313). Experts in the science of decision, given a vision of equity for the downtrodden, need little help from democracy. An elite of the good and the competent will be a better form of government. The role of the university in all of this is to produce such an administrative elite. Academic public administration should seek to attract and produce change agents or activists bent on securing greater equality regardless of congressional or presidential mandate.

This book is not concerned with empirical studies or explanations of administration (with the exception of the paper by Sharkansky). It is concerned with conceptual frameworks and might, therefore, have greater value to academicians than practitioners. It is, however, mostly concerned with philosophy and criticism of our governmental and, to a lesser extent, academic institutions. Consequently, for the most part, if it tells us anything it tells us about the emotional-intellectual state of several young teachers of public administration rather than about administration. Since they are teachers of administration, the book is best regarded as containing *facts in* administration rather than *facts about* administration.

It is customary for a book reviewer to give his personal evaluation of a book. While I can accept much of the analysis, the ideas involved in the "Minnowbrook Perspective" are difficult to evaluate. The positions in this "Perspective" are so fundamental that they are really beyond proof or disproof. Such fundamental commitments are acquired by an act of will. These positions are so contrary to my own as to render attempts at communication all but useless. I am committed to the belief in objective reality; the belief in the possibility of objective truth, hence knowledge; the belief in pluralistic democracy; the belief that administrators should be responsible to Congress, the President, and the Courts. I believe in tolerance, in negotiation and compromise, in contract, in incremental policy making. I do not understand why it has not occurred to the "New Public Administration" group that the governing instrument they wish to create might turn against them and their values and promote a set of values of an entirely different kind.

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The Political Persuaders: The Techniques of Modern Election Campaigns: By Dan Nimmo. (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1970. Pp. 214. $2.45.)

"Can the 'scientific politician' do better than his 'natural' opponent?" Robert Lane and David Sears promised we would have the answer to that question by 1974 (see their *Public Opinion*, p. 56).

Dan Nimmo, writing in 1970, and thus with four years, if he wants it, to change or firm up his mind, says the answer is becoming clear. The answer is yes and no, but mainly yes.

The scientific politician, Nimmo says, "does better" with the uninvolved citizen. He knows that knowledgeable citizens, politically interested citizens, and partisan, committed citizens (who are all probably the same people) cannot be converted, and can at best only be reinforced. The scientific politician knows that the citizens worth attending to are those who are less knowledgeable, less interested, and less committed. Because, as Converse very effectively demonstrated ten years ago ("Information Flow and the Stability of Partisan Attitudes," *POQ*, Winter, 1962), the least political are the most manipulable. They are, in addition, the hardest to get to, because they will not initiate a search for political opinions. To generate political opinions in them, whether they will or no, the scientific politician uses the techniques of the mass marketeer—impulse politics in place of impulse buying.

The principal virtue of Nimmo's book is that it is an up-to-date survey of techniques for getting to the least political. The central part of the book—chapters on "Campaign Management," "Profiling the Electorate," and "The Tactics of Mass Persuasion"—count the ways the least political are wooed and won by scientific politicians.

Around those central chapters Nimmo weaves a theory of "attitudinal and perceptual effects," The scientific politician, he says, finds partial and often nonpolitical linkages to voters, then minimally and temporarily politicizes them in the context of the campaign—just long enough and strong enough to secure the vote. The result is a discrete, unselfconscious, and impermanent, symbiosis of candidate and apoliticals in which the candidate gets the winning margin (he hopes) and the apolitical citizen keeps his self-respect (and obtains relief until the next time around). The mass man appears once more as one who, in political things, moves through perceptions to images to behaviors with little if any of the psychosocial integration that characterizes rational man. Thus:

Mass Man: Perceptions → Images → Behaviors
Rational Man: Beliefs ↔ Attitudes ↔ Behaviors

Once more social science seems to have shown the improbability of reasoned politics. Nimmo appears to believe that things are getting worse: campaigns are "increasingly" pseudo events (p. 26); there is an "increasingly growing" group of voters who rely on TV and "usually isolate themselves from all other media" (p. 118).

But *are* things getting worse? Campaigns were always pseudo events, were they not? The case could be made—convincingly, I think—that elections have always been unsubstantive and unthoughtful in direct proportion to the size of the electorate. If the American electorate now is becoming more knowledgeable and politically conscious (all the correlates of education, age, and class tell us that it is), then things may not be getting worse. We don't know. The trends are not clear yet in the contest between manipulative technology and campaigns in which there is a shared and genuine public discourse. I think we're getting more of both.

An end note. With a minimum of editing, this could have been a trade book. It may not be too late even now. It might sell five thousand copies a year in airport bookstalls.

BERNARD HENNESSY
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The Politics of American Foreign Aid. By Michael Kent O'Leary. (New York: Atherton Press, 1967. Pp. 135. $6.95.)

Winston Churchill once said that democracy is the worst form of government "except for all the others." Professor O'Leary says that the man on the street evaluates foreign aid in somewhat the same way: ". . . foreign aid is a good policy except for all the forms in which it has been applied" (p. 113). This aphorism epitomizes the ambiguity surrounding foreign aid and which affects every level of U.S. policy making and administration. But Professor O'Leary has achieved an academic tour de force by writing a different book on a well-worn subject. This one concerns the *domestic politics* of foreign aid.

The author notes that experts disagree about where trade ends and aid begins. He compounds this confusion by introducing a new term, "ideological aid." Harking back to Tom Paine, he writes that "in [this] sense, America has been in the business of foreign aid for its entire history" (p. 5). Moreover, "much of the malaise about foreign aid can be attributed to this ambiguity" about its nature and purpose.

The foreign aid dilemma is not just a case of ambiguity however, not just the well-known American desire to be compassionate and hard-headed at the same time. Rather, and this is the central message, our political system has a capacity for producing a continuing stalemate on this question.

The book is devoted to an analysis of the workings of the entire political system, but to the reviewer the most striking evidence is contained in Chapter 3, "The Political Distribution of Opinion." Here the author states that "relevant opinion is distributed throughout the population in such a way that almost equal weight is given to the proponents and opponents of American foreign aid policy . . . neither . . . enjoy[s] significant political advantages" (pp. 29–30).

The geographical distribution of opinion shows a high incidence in all regions of "internationalism." This finding supports the national average—51 per cent—in favor of foreign aid (AIPO poll, March 1958). However, the South in the same poll registered only 38 per cent in favor. The South favors military aid for its direct opposition to communism; it also favors aid of a low-cost and clear-cut "missionary" character. Nevertheless, Southerners also see aid as a big spending program launched by the distrusted central authority that is forcing integration upon them. This program is carried out in favor of foreign peoples and leaders who happen to be colored. The intensity of negative feelings about the latter clearly override the favorable feelings toward the former.

The real possibilities for stalemate are seen in the fact that those states having the lowest approval of foreign aid (44 per cent in 1958) sent to Congress 57 per cent of Representatives and 52 per cent of Senators. In this case a nationwide minority opposing aid is represented by a majority in each house of Congress.

Favorable opinion within socioeconomic groupings is strongly related, not surprisingly, to high levels of education and income. Favorable opinions exceeding 60 per cent were found only in groups who had at least a partial college education (17 per cent of the population) and within professional, executive, and sales classifications (29 per cent of the work force).

Further possibilities for stalemate also exist in socioeconomic groups: Some anti-aid opinions are held with greater intensity than pro-aid beliefs. Furthermore, the author points out that "a little learning is a dangerous thing for foreign aid" (p. 35). More people in a middle position in education, having partially completed high school, considered foreign aid expendable —in response to a question about which items of government expenditure they would most like to see reduced—than did those with either more, or less, schooling. As indicated, one of the contributions of the study is its emphasis not just on the existence of opposing opinions but on an analysis of the intensity with which opposing opinions are held.

When the results of poll questions on foreign aid are tabulated according to party allegiance in presidential elections, no significant differences are found (page 42). This observation is supported by polls going back to 1948. Reasons: first, on foreign aid, only one respondent in five had simultaneously (1) an opinion, (2) any knowledge of government policy, and (3) an accurate perception of the policy positions of the two parties. Second, higher levels of voter knowledge and interest tend to be associated with "bread and butter" issues. (But the author notes that the single issue in 1960 on which the greatest number of respondents showed knowledge and perception of party positions was the question of taking a firm stand against China and the Soviet Union.) Third, foreign aid offers no opportunity for candidates to tap a consistent set of opinions.

The attitudes necessary for support of foreign aid are found to include: internationalism, support for long-term involvements abroad, a belief in the "exportability" of the "American way of life," tolerance of "deviant" international behavior by aid recipients, and a certain optimism about government spending (p. 43). These values are diffused throughout American society and are "not concentrated in a politically effective manner." This makes foreign aid a very tricky issue for presidential candidates.

For much the same reason, pressure groups have not been very effective either in marshaling their publics in support of foreign aid or in providing alternatives. Foreign aid is thus decided largely by the interplay between the Executive and Congress. In his treatment of Congress, O'Leary finds that there *are* party differences between Democrats and Republicans. In the 86th and 87th Congresses, in the House the average Democrat voted supporting foreign aid 68 per cent of the time; for Republicans it was 54 per cent. In the Senate, the average figures for Democrats and Republicans were 53 and 48 per cent, respectively. Republicans on the whole have been more critical of foreign aid and more willing to restrict its operations and reduce its size.

Congressional voting is, however, much affected by which party has the Presidency. Senators and Representatives shift their votes on foreign aid significantly between one Congress and the next; both parties exhibit strong patterns of consensus when voting with a President of their own party; out-party votes in both houses exhibit bimodal distributions associated with well known patterns of conflict.

The Presidency is thus the hub of public-opinion formation on foreign aid. It is here that the major policies have been established—the Marshall Plan, Point IV, the Alliance for Progress, the Peace Corps. Yet the tools available to the Presidency in the continuing contest for support are only partially effective. The frequent resort to "reorganization," the shuffling in and out of new administrators, the appointment of citizen and expert study commissions, while all intended to impress the public with presidential concern and hard-headedness, have confused the public and have reduced AID administration to musical chairs. The result is a weakening of presidential direction with Congress moving into the vacuum. At times, a confusing role reversal occurs with the Presidency emerging as important in the setting of policies, and the Congress becoming primarily embroiled with details of administration. A sort of political Gresham's law seems to be at work here.

O'Leary has an interesting appendix on the "ranking of values" with respect to foreign aid. By comparing the poll results over a fifteen-year period, with respect to two or more questions having one element in common, the author was able to rank the strengths of the positive and negative factors in public opinion relative to foreign aid. The methodology utilized here is inventive and deserves further experimentation; it tends to overcome some of the weaknesses of research based on public opinion polls. Namely, it deals with the continuities in public opinion over a relatively long period and allows for qualitative and comparative statements about the apparent underlying values involved. In spite of this the present reader found the specific results, in this case, rather unconvincing.

Professor O'Leary has done us all a favor in his well-informed, data-packed, and occasionally witty book. His book has "added to knowledge" and while it has ended on the same note of perplexity with which it began, that, after all, is typical of work on foreign aid.

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The Politics of Fiscal Policy Formation. By Lawrence C. Pierce. (Pacific Palisades: Goodyear Publishing Co., Inc., 1971. Pp. 224. $8.00.)

Lawrence C. Pierce has written a good book on *The Politics of Fiscal Policy Formation.* Anyone who is interested in how fiscal policy is made in America should begin with this volume. I have a few qualms about it; the first half, in my opinion, is considerably better than the second. Taken as a whole, however, the book represents an admirable achievement from which I learned a good deal.

Pierce seeks to connect the application of economic theory and data to the organization of advice on fiscal policy in a political context. Through extensive interviews with participants, he describes in operational terms how economic forecasts are made, how problems are determined to exist, and how the various presidential and congressional actors go about resolving them. He is particularly good at revealing the relationships between the fiscal policy instruments—taxing, borrowing, spending, lending, etc.—and the political costs involved in using them to help secure such policy goals as price stability, full employment, and economic growth. Since the executive has certain discretionary powers in the area of debt management, for instance, it may use them, when a change in disposable income through taxes would be preferable, because the more desirable alternatives require cooperation from others who are not always willing to give it.

The admixture of political and economic considerations leads to the adoption of informal norms that may be more important than any declared policy. Social security taxes are often favored for adjustments, for instance, because people do not seem to react strongly to speed-ups or postponements of increased benefits or levels of taxes. Tax increases should not be sought, another norm states, unless the budget adjustment is at least $2 billion because the hassle is not worth it. Old taxes are apparently better than new ones because people are used to them. So if a tax is scheduled for removal, the first alternative would be to consider continuing it. I would have liked to see more done with the practical impact of these norms under different conditions.

The third and fourth chapters on economic forecasting are the best in the book. Pierce describes how the Troika (The Council of Economic Advisors, the Treasury, and the Bureau of the Budget) goes about making economic forecasts. Each major participant has its own biases. The Treasury, which is always worried about having to raise taxes, tends to underestimate actual revenue. The Bureau of the Budget, which owes its main allegiance to the President but must also get something for the various spending departments, tends to underestimate expenditures. At once, this permits the BOB to give the President what he wants, which is the appearance of less of a deficit than supposed, and to enable a few more expenditures to appear in the budget than would otherwise have been the case. The Council, composed of academic economists, shares their preference for greater concern over employment rather than inflation and towards greater rather than lesser economic growth.

Members of all three organizations must get together, agree on a central economic story, resolve on a step-by-step basis the numerous uncertain aspects of each forecast, and arrive at a series of recommendations to the President with which all of them can live. Disagreement may be found in qualifiers to various sentences or in the shadings of words. When greater uncertainty than usual is felt to be present, or when the President reaches out more widely for conflicting opinions, advice may be sought from academic economists around the country.

Throughout the book, Pierce provides a description and running commentary on President Johnson's tax surcharge of 1968. By December 1965, it had become apparent to the Council of Economic Advisors that Vietnam war expenditures, in the context of an economy operating at near full employment, would increase inflationary pressures unless there was a significant increase in taxes. President Johnson did not want to cut down on his domestic programs, and he found support in Congress for an increase virtually nonexistent. At one point he asked members of the Council whether he should, nevertheless, still ask for a big tax increase. Pierce reports the following exchange:

> 'Should I be on the right side for the good of future policy even if I would lose the fight?' After much deliberation, we told him that it would probably be better not to ask for it, that from the psychological standpoint, the costs of a loss of faith in the 'new economics' would be less than the cost of a loss of confidence in the financial markets at home and abroad arising out of the president's inability to get such a request enacted (p. 122).

Pierce does not ask what business "expert" economists have in making recommendations to presidents "from the psychological standpoint," but it would have been a good question.

The second half of the book is devoted to chapters on the Presidency and Congress, ending with a series of propositions purporting to describe fiscal policy formation. Somehow, there is a loss of direction. Little new is added, and much old is recast in unsatisfactory form. What is one to make of propositions like "The greater the degree of technical complexity, the more difficult it is to predict policy outcomes" (p. 183), or "The greater the expected impact, the larger the number of issues and goals likely to be evoked" (p. 187)? Similarly, in the chapter on the Presidency, Pierce's taxonomy of role concepts in decision making (general leadership roles, special staff roles, critical problem roles, etc.) is not at all illuminating. By the end of the book the author is concerned with all sorts of questions—whether policy making will be more or less comprehensive or incremental, whether a smaller or greater number of alternatives will be considered, whether the decisions will be made more slowly or rapidly—that neither he nor anyone else can answer. He would have been better off following through on his earlier efforts to show how the standard operating procedures, which evolve from the interaction of economic analysis, political requirements and organizational needs, affect a variety of fiscal policy decisions.

Nevertheless, I recommend the book highly to students of national politics and public policy. It is the best we have on the political organization of fiscal policy.

AARON WILDAVSKY
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Aussenpolitik in Adenauer's Kanzlerdemokratie. By Arnulf Baring. (Munich and Vienna: R. Oldenbourg Verlag, 1969. Pp. 492. Brosch. DM 28, Leinen DM 38.)

Although Baring's work is subtitled "Bonn's Contribution to the European Defense Community," neither the title nor subtitle reveals that the book is actually an exhaustive case study of the beginning of Adenauer's efforts to rearm West Germany and to integrate her into the Western camp. Baring records the relevant history of the period 1949–1953 using an explicitly German-centered, decision-reporting format. Within the limits he has set for himself, he has done a thorough, scholarly piece of work that deepens our understanding of Adenauer's decision-making process, the nature of West German federal-state relations in the early 1950s, and, most persuasively of all, the extremely narrow and illiberal conception of democracy held by Adenauer and most of his chief assistants.

While he has written a good case study, I would criticize Baring for not having written a book to go with the title. For example, there are very few generalizations about foreign policy that transcend the first four years of Adenauer's rule. Baring offers no proof that decision-making characteristics he discerned at an early stage in Adenauer's rule were continued past the first four years. Complicating these omissions is the fact that the thesis of the book is first stated in an appendix. Before one reaches that appendix the reader must guess at exactly what it is that Baring is trying to prove.

His thesis is, first, that the particular contribution of West Germany as an actor in post-World War II European politics has not been duly recognized by Western writers (he singles out as an example Hanrieder's *West German Foreign Policy, 1949–1963*), who overemphasize West German passivity and acquiescence. Second, he argues that Adenauer's success in achieving rearmament (despite majority opposition) and integration with the West (despite warnings that this would result in permanently dividing Germany) occurred because there was no effective domestic political power opposing Adenauer.

In trying to prove his twofold thesis, Baring reaches a number of interesting conclusions about German political actors of that period. Theodor Heuss, for example, who has been depicted frequently as a forthright democrat setting examples of proper behavior for more authoritarian Germans, appears in Baring's study as an acquiescent follower of the Steel Chancellor. The present President of the Federal Republic, Gustav Heinemann, on the other hand, receives favorable notice as one of the very few principled opponents of Adenauer who resigned a ministerial post because of opposition to governing policies.

The focus of the book is on Adenauer as policy maker: his conceptions, strategy, and tactics which dominated the West German political scene. Baring uses a set of themes familiar to readers of Adenauer's biographies in explaining Adenauer's very early decision to anchor West Germany in the West. Baring's explanation is more persuasive to this reader than those offered by Hiscocks and Wighton, for example, because of his more scholarly documentation and the use of deathbed confessions that occurred after the Hiscocks and Wighton books had been written. Using these more persuasive data carefully, Baring links factors such as the Rhineland's ties with the West and Adenauer's Catholicism, his anti-Prussian and his anti-Communist beliefs in a way that convincingly supports even the sweeping statement that "National Socialism, the war, and defeat could not change the foreign policy conception of Adenauer" (p. 56). Baring's study furthermore reveals that Adenauer's early proposals for Western European unification (Fall, 1945) were designed to make impossible a future war between Germany and the West by fusing the economic power of what was to become the ECSC several years later.

Baring claims that there was a "vacuum of domestic political power" (p. 339) when Adenauer's foreign policy course was revealed. There were those who sought to change the Western integration policy but they had no political power with which to challenge Adenauer. Opponents also are shown to have forgotten Adenauer's dictum that "policy must be simple" and thus to have failed to produce popularly acceptable alternatives to Adenauer's policy. Adenauer monopolized the foreign policy-making process by his recruitment of experienced men like Globke and Lenz who led a "political General Staff" which in turn created a foreign policy bureaucracy insulated from the government, the parliamentary majority, the opposition, and public opinion.

This reviewer was particularly struck by the importance of authoritarian expectations in Adenauer's method of governance. Adenauer and the majority of his top-level assistants appear to have been truly ignorant of the nature of an open, liberal democracy. Adenauer's near total control over subordinates in his own Chancellor's Office was apparently the way he thought publishers and radio station heads, for example, controlled their subordinates. Adenauer acknowledged no spontaneous, independent criticism: all was opposition-inspired, politically motivated. Hence in the field of foreign affairs, anyone in West Germany who advocated German neutralization and demilitarization in the early 1950s was regarded by Adenauer as "either an idiot of the highest order or a traitor" (p. 107). Adenauer and his chief supporters in the government publicly questioned the motives and integrity of the judges on the federal Constitutional Court when that court acted within its competence but in a way that threatened to defeat Adenauer's treaty proposal. Similar charges were hurled at the Bundesrat when it sought to review the treaties involved in the European Defense Community negotiations.

Baring supplies additional evidence to that given in earlier sources on Adenauer's actual reasons for maintaining the position that West Germany and the West should only negotiate with the Soviets "from a position of strength." He offers considerable evidence that Adenauer had actually abandoned the eastern part of Germany to Soviet hegemony on grounds of *Realpolitik* as well as from the fear that the West would be made uneasy by any serious attempts at reunification.

Baring's study, which includes a useful annotated bibliography, is, in conclusion, a well-documented case study of a most important and formative set of negotiations in the beginnings of Adenauer's chancellorship. I hope that Baring will return at some future time to write the more general work this book's title suggests.

PAUL J. CASSIDY
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Attitudes et comportements politiques à Boulogne-Billancourt: enquête par panel 1965–1967. By Frédéric Bon and Guy Michelat. (Paris: A. Colin, 1971. Fondation Nationale des Sciences Politiques: Travaux et recherches de science politique, No. 12. Pp. 129. No price indicated.)

Boulogne-Billancourt, a residential and industrial suburb of about 110,000 inhabitants on the southwest corner of Paris, features the immense government-owned Renault automobile factory along with luxury apartments on the edge of the Bois de Boulogne. The Fondation Nationale des Sciences Politiques has devoted considerable study to this microcosm of political and social attitudes (cf. E. Findberg, *Evolution politique et sociale de Boulogne-Billancourt depuis 1945,* Paris: F.N.S.P., 1968).

The present work is the result of an extensive questionnaire (10 pages of close type) given to 593 respondents in 1967 after the elections to the National Assembly. All the subjects had also been similarly questioned in 1965 on the occasion of the presidential election (de Gaulle won, remember?). On the basis of these two sets of responses, Messrs. Bon and Michelat draw the most extensive and tenuous conclusions regarding consistency and change in voting opinions and patterns with reference to: sex (not very significant), age (younger voters change more often and prefer presidential system over parliamentary), education (pattern more consistent among people who went beyond primary schooling), occupation (voting stability among upper cadres), income (change is more frequent if real income is either rising or falling), television in home (no significant effect on voting patterns—N.B., French TV is state-owned and -controlled), interest of respondent (greater consistency of voting patterns among those with greater political interest, as measured by their reading of political news items, listening to political broadcasts, etc.), and so forth, with some very nice charts and tables.

A major portion of the analysis is devoted to the question of the Gaullist presidential system versus the parliamentary type: More men than women favor the latter (49 per cent of men respondents as against 38 per cent of women), except that this variation disappears when the political-interest factor is held "high" for both men and women, in which case 57 and 56 per cent, respectively, favor the parliamentary form. Support of the Gaullist system varies with income level and with education (i.e., lower classes like strong rule). Among respondents aged 50–64, 61 per cent prefer the presidential system, while those aged between 21–49 are almost evenly divided between the two systems in their preferential opinions.

In a final chapter the authors attempt to assess the problem of attitude stability, in general, among voters. Conclusion: The greater the integration of the voter into the political system, either because he is a member of a political party or because he is part of the educated elite, the greater will be the consistency of his political attitudes and behavior. Conversely, the more disaffected and ignorant the voter is, the more variable and apathetic will be his voting behavior. (The argument is really more sophisticated than this, but I am not sure it is more significant.)

For those readers interested in knowing the breakdown of both the 1965 and 1967 samples, the figures are given by sex, age, level of education, income (five categories), occupation of respondent (eight categories), and occupation of head of household. The structure of the two samples varied at most by 5 per cent (in sex differential and in income levels), with a mean variation of only 1 per cent. How the two samples compare with the occupational structure of Boulogne-Billancourt is not indicated, but one notes that 25 per cent of the respondents belonged to the professional and middle-to-upper cadres, whereas only 12 per cent of the active population of France as a whole belonged to this category in the 1962 census; conversely, 35 per cent of the respondents were in the employe-worker-service category, whereas 55 per cent of the active population of France, and 75 per cent of the active population of the Parisian suburbs, were in this category.

To draw from this study any conclusions other than those concerning voter change and stability, mentioned above, would therefore be unwise. But, although not earth-shaking, those data will be of interest to all students of voting behavior and opinion.

WESLEY D. CAMP
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Lenin, Gosudarstvo, Politika. By F. M. Burlatskii. (Moscow: Izdatel'stvo "Nauka," 1970. Pp. 498. 2 rubles, 40 kopecks.)

It is extremely difficult for a Westerner to review any Soviet publication. One can never be sure of the author's views: while a writer's remarks sometimes are his own, often they simply reflect the current party line. As the political atmosphere in the USSR changes, scholars (and other writers) are allowed more or less latitude to express their own convictions. Moreover, Western analysts often are too strongly inclined to look for hidden meanings or subtle protests against the regime or its leaders. Even those who are unenthusiastic about Kremlinology sometimes see Aesopian significance in perfectly innocuous statements.

The present book shows how relevant these problems are. Professor Burlatskii, trained in philosophy, is best known to American political scientists as a vigorous and articulate advocate of the scientific study of politics. In a major *Pravda* article in 1965, Burlatskii advanced the radical notion that political analysis in the USSR involved little more than rhetoric and doctrinal exegesis. A genuinely scientific approach, he argued, involves "the asking of questions whose answers are not known in advance." Unfortunately, events have shown that the Brezhnev-Kosygin regime is extremely hostile toward freedom of scientific inquiry, and the promise of Burlatskii's article has not been fulfilled. Advocates of political science in the USSR are stifled by powerful conservative forces—academic, ideological, and political.

Burlatskii's book shows only too clearly the terrible constraints under which Soviet social scientists labor. The most interesting and innovative section of the book, "Society and Its Political Structure," is essentially a theoretical and methodological essay. It sets forth the rationale underlying a systems approach to the study of politics and argues for empirical research efforts combining structural and functional analysis. Burlatskii's presentation is almost entirely straightforward and undogmatic. He is clearly critical of the ossified state of Soviet social science. To bring it up to date, Burlatskii recommends the "critical assimilation" of "bourgeois" research techniques, especially modeling and statistical methods (p. 186). He explicitly urges Soviet sociologists to expand their research into political matters and criticizes official agencies for their failure to make available certain of their data.

Burlatskii emphasizes the instrumental role political science could play in the USSR. To bolster his argument, he cites the experience of East European social scientists in developing an independent discipline of political science, e.g., in Poland and Yugoslavia. Moreover, he sets forth some rather unorthodox proposals. He supports the practice, adopted by a number of East European states, of allowing contested elections for legislative seats. He even repeats the extraordinary suggestion for court reform he first put forth in 1958, to increase the number of people's assessors from two to ten and thereby introduce a form of trial by jury.

The bulk of the book, however, is tendentious and polemical, hardly conforming to the innovative suggestions sketched out in the methodological section. Burlatskii's views on most subjects are predictable and dogmatic. His criticism of "bourgeois" (primarily American) political science is sweeping, sometimes to the point of being doctrinaire. According to Burlatskii, political science fulfills very explicit functions in bourgeois society. It rationalizes the existing system of property and power relations and contributes to the political socialization of Western youth. This conception of political science is important and merits careful analysis;

indeed, some Westerners hold the same view. But Burlatskii makes no attempt to prove his point; he advances certain conclusions without providing any evidence whatsoever. Thus, he says, American political science developed "under the influence . . . of state monopoly capitalism" as a response to Marxism and to help the "enormous state machine" function more efficiently. Even now there is a "direct link" between the discipline and state-monopoly capitalism. And political scientists are responsible for the "ideological and political training of students"—all of whom, he says, must attend a series of lectures on political science. The purpose is to train citizens "in a spirit of defending bourgeois ideology and combatting communism and socialism" (p. 185).

But if Burlatskii is no radical, he certainly is no hack. His writing is far superior to most Soviet writing on politics. His defense of the Marxist approach to the study of politics and his criticism of Western behavioralism (especially the group study of politics) are intelligent and articulate. But the reader is left frustrated. Burlatskii is unquestionably an able and informed scholar. His enthusiasm for methodological rigor, for empirical research, for a comparative approach, is clear. He is manifestly impatient with the need to conform to an official line; on the basis of his earlier writings, it is clear that he is a progressive thinker. Unfortunately, this book does not meet his own high standards. It is a casualty of political change in the USSR, of a regime which is increasingly unenthusiastic about academic freedom. Perhaps if he had published it earlier (it was ten years in the writing), Burlatskii would have produced a very different manuscript. But Soviet censors today are even more vigilant in preventing the publication of anything resembling value-free social science. Though not without considerable merit, the book ultimately is disappointing.

DAVID E. POWELL

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Memoiren 1918–1934. By Heinrich Brüning. (Stuttgart: Deutsche Verlagsanstalt, 1970. Pp. 721. Price not listed.)

While Heinrich Brüning's memoirs cover the years 1918 to 1934, three-fourths of the book is devoted to his work as *Reich* Chancellor, 1930 to 1932, and as Foreign Minister, 1931 to 1932. Some seventy pages deal with the two years preceding his flight to Switzerland in 1934. His years as a youth are not mentioned. A few pages are given over to his experiences as a front line soldier.

The memoirs were written in several parts, in different years, beginning 1934 to 1935 in Switzerland and ending in the 'fifties when he wrote in the United States about the period from 1919 to 1929. His book is based on a wartime diary, newspaper articles published by him and some friends in 1923, and, for the crucial years of the Chancellorship, on the detailed daily appointment calendar notes (*Tageszettel*) kept by State Secretary H. Pünder, Chief of the Chancellery, and supplemented by Brüning's personal stenographic notes. The *Tageszettel* and a number of important documents were smuggled out of Germany in 1934.

The memoirs provide only a few attempts at an overview of the historical events of the two-year period shortly before Hitler's rise to power. They cover in some detail a large number of grave economic and political problems, domestic and foreign. Among them were reparations, military and naval questions, the *Osthilfe,* and the Austrian customs union; financial crises in industry, agriculture, banks, newspapers, and religious and other organizations; widespread corruption and fraud in public and private life (pp. 406–409, 449–50); the waste of public funds, unemployment, elections, and relations between the *Reich* and the *Länder.* The fateful emergency decrees are described and discussed. Brüning had in readiness drafts of additional emergency decrees which he was secretly prepared to use for the sake of *"Ruhe und Ordnung"* in the event that coalition negotiations in the *Länder* ended in failure. These drafts provided for the *Reich* to take over from the *Länder* control of the police and administration of justice (p. 570).

Unfortunately, Brüning offers no philosophical discussion of the use of emergency decrees, no coherent examination of the role of Hindenburg or of Brüning's attitude toward him, and no questioning of whether the system of proportional representation contributed heavily to the German political party mess. He does, however, provide a number of acute observations on German political life of the time.

Brüning had some trouble keeping his Foreign Office in line (pp. 295–97). His official telephones were bugged by *Reichswehr* officers and secret information was passed on to both General Schleicher and the Nazis. Secret Chancellery documents were made available to Berlin newspapers (pp. 395–98).

Brüning was a man of strong convictions, and of an independent mind. He was a convinced monarchist who favored and planned for the restoration of the monarchy (pp. 373, 378, 418, 453, 456, 462–63, 579), but he often praises Social Democratic leaders for their courage and sense of civic responsibility (pp. 110, 132, 214, 218, 260). Although he was a strong conservative, he frequently castigates the parties of the right, the *Reichswehr*, big industrialists, bankers, and land owners. A devoted Centrist, he had bad personal and political relations with Prelate Ludwig Kaas, the national leader of his party. He writes bitterly about the willingness of many Centrists to play ball with the Nazis, some, he claims, to save their cabinet jobs in the *Länder*. He even charges prominent pro-Nazi Centrists with "treason" against the party (pp. 668–69). On an official visit to Rome, he tells us that he felt close to Mussolini and had sharp differences with Cardinal Pacelli, Papal Secretary of State. He told Pacelli that he "would have to resist any attempt on the part of the Vatican to exercise influence on his political decisions or the attitude of the Center Party," but he felt free to advise the Pope that "National Socialism and Fascism had no spiritual bases in common" and that he, therefore, "considered it necessary that the Vatican come to a conciliation with Mussolini at a time when Bolshevist ideas in red or another color were spreading throughout the world" (pp. 354–60).

Brüning was bitterly disappointed in the weak and changeable attitude of most German bishops and many higher clergy toward the Nazis (pp. 653, 664–65, 673). He expresses horror at the willingness of the Vatican to conclude an unfavorable concordat with the Third Reich and at its unwillingness to form a common front with Protestant believers in defense of basic Christian teachings (pp. 666, 670–73, 677).

The memoirs abound in detail. Curiously, there are many references to meetings taking place on "Saturday" or "Wednesday at 9:30 A.M." but few indications of the day of the month, or year. There is an overabundance of last names, often without indication of positions held or other background information. A valuable but incomplete Index of Persons helps to identify many people of importance but falls short of clarifying Brüning's references to "a high official," "his uncle," or "the mother-in-law" of an ambassador.

For the student of German government and politics there is plenty of "pay dirt" in Chancellor Brüning's memoirs, but the individual veins vary greatly in worth. The prospective publication of a number of Brüning's university lectures and letters may be expected to add to the value of these memoirs by clarifying a number of questions.

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The British General Election of 1970. By David Butler and Michael Pinto-Duschinsky. (London and New York: Macmillan, St. Martin's Press, 1971. Pp. xvi, 493. $15.00.)

This treatise, which is the eighth in the series of Nuffield College election studies, follows the main contours of the three preceding volumes, but with a few beneficial variations. In place of the sketches of the campaign in selected constituencies, the authors offer a more general and better-ordered treatment of electoral politics in the localities. Interviews over a wider range have yielded more information about developments in the interim between elections and about strategies in the national campaign, as well as political activities at the local level. Included for the first time are valuable census data arranged on a constituency basis.

The authors set for themselves three "central tasks": (1) to see why the fortunes of the Labour Party dwindled so rapidly between 1966–1969; (2) to show how it staged a comeback in 1969–1970; and (3) to "examine the way in which the Conservatives won after all" (p. ix). These questions, though oriented more toward description than theoretical analysis, are nevertheless clear-cut. The study, however, is not explicitly organized around the questions, and at times the reader has to pull together factors that are buried at various places in the narrative. Collecting a vast amount of useful factual material, the authors intend their study to be primarily descriptive: "We hope we have made a substantial contribution to the argument and provided evidence which proponents of any point of view can cite" (p. xiv).

After wrapping up the voting charts which recorded their 1966 victory, the members of the Labour Government turned to the economic graphs which indicated nothing but trouble. To counteract inflationary pressures and chronic runs on the pound, they instituted wage and price controls. Eventually they had to resort to devaluation, but were slow to take advantage of it. As unemployment spread, deflationary measures grew more unpopular. Strikes in 1968 prompted the Government to introduce an Industrial Relations Bill which generated so much hostility within the Party that the minister was forced to withdraw it. Dissatisfaction with the Government's performance in

1966–1969 was reflected in the opinion polls (its support dropped to 26 per cent at one point), heavy losses in by-elections and local elections, and a decline in Party membership. The wounds inflicted by Labour's economic policies, the authors conclude, were not sufficiently healed in time for a June election.

In the autumn of 1969, however, Labour's position began to improve. The balance of payments turned in Britain's favor, and the Government scored points in its handling of the violence in Northern Ireland. Encouraged by these developments, Labour strategists launched a campaign to rally the Party activists, and the Government relaxed some economic controls. After spotty improvement in by-elections and county elections, Labour registered dramatic gains in contests for borough council seats in May 1970, an increase in popular support that was confirmed by the polls. At this point the Prime Minister opted for a June election rather than waiting for October. As the campaign advanced, the odds were strongly in Labour's favor.

In light of these developments, why did the Conservatives win? The authors find part of the answer by comparing the two party organizations. After 1966, the Labour organization suffered from a further loss of agents, a rift between Transport House and Downing Street, and low morale among the activists owing to defeats in local elections and hostility encountered on the doorsteps. Labour's late recovery and an early election did not allow sufficient time for the Party managers to get their machines in good working order.

In the period between the elections, the Tories also faced difficult problems: internal disagreements, a narrowed range of maneuver in attacking Labour policies, and a leader who did not inspire confidence inside or outside the Party. But soon after the 1966 defeat, Conservative leaders set about to build up and streamline their organization. Increased financial resources enabled them to upgrade the agency service and to undertake research programs as a basis for policy discussions. They also tried to recruit members and candidates from a wider social base, and they designated certain marginal seats for special effort. While most of these projects fell short of the targets, the Tory organization at least did not deteriorate, and the Party's managers were better prepared for an early election than were their competitors.

In the authors' view, the outcome—for the first time in many years—was probably decided by the campaign, even though it did not generate much enthusiasm. Issues were not conspicuous, and media coverage focused upon Wilson and Heath. Recognizing increased volatility of public opinion in recent years, the authors suggest the possibility of a swing to the Tories in the campaign's closing days. But what caused it is still a mystery (pp. 186–187). They see the volatility as a long-term trend, with voters being swayed by income boosts timed so that an election can be held before prices catch up.

For the first time in a general election, eighteen-year-olds voted, and candidates were permitted to list their party identification on the ballots. The voter turnout, however, was the lowest since 1935, partly as a result of holidays. The authors found no relationship between diminished turnout and swings to the Tories—an analysis confirmed by survey research. Abstention rates were especially high among Liberal voters. The evidence suggests a higher turnout among colored immigrants who may have been activated by "Powellism," and the Conservatives derived little gain from local feeling against immigrants. According to the analysis, the nationalist parties in Scotland and Wales drew support from people who are ordinarily nonvoters.

The swing—the biggest in the postwar era—was more uniform than usual. In analyzing the swing patterns, the authors detect mostly cumulative trends rather than pendulum movements, with Labour exhibiting strength in Wales and Scotland, the large cities, and parts of the North, and the Conservatives drawing their support from rural and suburban areas, parts of the Midlands, and the South.

Timeliness dictated that the study be published as soon as possible, and, despite this pressure, the authors have done a fine job of reporting events and compiling statistical information. Understandably, there are occasional slip-ups, as when they report that the April local elections both "offered little comfort to Labour" and showed a "marked improvement for Labour" (pp. 131, 172). Scholars who are looking for a chronicle of developments before and events during the campaign (including an interesting treatment of Scottish nationalism) will find this a valuable book written in an engaging style. Researchers, too, will find the statistical data a very useful source; especially important are the census data on the constituencies.

Scholars with a thirst for hypothesis testing and theory building will want different questions to be posed within a conceptual framework and will urge a refinement of the rich data. The variables employed are usually constituency and party, and the aggregate data provide only crude measures of individual voting behavior. In the absence of survey research, for example, we cannot be sure that it was the Liberal vote that switched to the nationalists. By the same token, the "swing" is a measure of net voting change, but it can only suggest clues as to which voters shifted position to produce that change. Appendix III goes a step further by introducing a socioeconomic dimension, and it lays down the challenge for future researchers to combine the new census materials with available survey research data.

Specialists in British politics and research workers in the expanding vineyard of comparative electoral behavior will want to have this volume on their shelves as a reference and source book. Indeed, this reviewer purchased it and enjoyed reading it even before he was awarded a free copy for writing this critique.

John E. Turner

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The Elites of Barotseland 1878–1969: A Political History of Zambia's Western Province. By Gerald L. Caplan. (Berkeley and Los Angeles: University of California Press, 1970. Pp. 270. $8.50.)

The key figure in the first—and perhaps the best—part of this study is Lewanika, the King of Barotseland. Lewanika's cooperation with the whites led to his kingdom acquiring a privileged status in Northern Rhodesia, of which account had to be taken in the negotiations which preceded Zambian independence in 1964. Dr. Caplan argues persuasively that Lewanika followed two overriding policies. The first was to consolidate his own position and to do this he turned for support to the British South Africa Company. Second, he sought with the help of the whites to secure the safety and modernization of his kingdom. His success in both directions, however, was equivocal.

Yeta, who succeeded Lewanika in 1916, belonged to the new, educated elite which had emerged in Barotseland and led its members in an alliance with the traditional ruling class against white rule. In acting in this way rather than turning to, say, Native Welfare Associations "based on a wider nationalism," Dr. Caplan argues that "Lozi nationalism was . . . essentially reactionary, an attempt to restore the lost privileges of the past" (p. 122). But this is to argue with the benefit of hindsight. In 1916 it was hardly "reactionary" to seek to preserve the Lozi nation against further white encroachment or to fail to see that it was in Barotseland's best interest to become one day a province of an independent Zambia. Obviously, it is important to define the term "nationalism." According to Ginsberg, the "nation" is "a group whose members are held together by a sense of community, induced and sustained by objective factors which, however, vary from case to case;" these objective factors may include language, religion, race, and common traditions and customs. On this definition, it is clear that Barotseland qualified as a nation in the earlier part of this century no less well than the artificial unit of Northern Rhodesia, which was still in the process of creation.

The story unfolds chronologically. In the second half of Yeta's reign, after the Crown had succeeded the Company in 1924, the Lozi ruling class realized that it would not recover its former powers and authority. It decided instead to be satisfied with material manifestations of its status, with conspicuous consumption compensating for the absence of real power. And so Barotseland became a "living museum," a political and economic backwater, cut off from the current of African politics in the rest of Northern Rhodesia. This remained the situation under Yeta's successor, Mwanawina (1948–68), the overriding purpose of whose reign was to preserve those few privileges which remained to the traditional ruling class. In these circumstances, the torchbearers of the progressive interests of Barotseland became in the 1930s a handful of young men on the Copperbelt and along the rail line, and subsequently the Barotse National Association and the Lozi nationalists within UNIP.

The author shows, however, that the determination to preserve the status quo and to maintain the special position of Barotseland within Northern Rhodesia was not the preoccupation of the traditional elite alone. For even UNIP in Barotseland comprised men "who intended to be both Lozi patriots—tribalists—and nationalists" (pp. 203–4). But so, too (and this significant fact the author does not mention) did UNIP's Lozi national leaders. The latter, according to Mulford, threatened in January 1964 "to withdraw from the party altogether, unless they were granted what they regarded as adequate representation in the new Cabinet." (*Zambia: The Politics of Independence*, pp. 329–30.) Mulford's example underlines again the difficulty which arises from Dr. Caplan's failure to define one of his key terms—in this case "tribalism." He equates parochialism with tribalism (p. 134), thus ignoring the fact that a people may react in a parochial way without necessarily being tribalist. Following James S. Coleman and Carl G. Rosberg, Jr., (*Political Parties and National Integration in Tropical Africa* [Berkeley and Los Angeles: University of California Press, 1964], p. 687), we may take tribalism to entail "the persistence—indeed, the paramountcy—of 'primordial' attachments or ties" (p. 687). Are we therefore to conclude that in the period immediately before and after independence the Lozi were stirred solely by an emotional attachment to the past? The author himself shows that this was not the case in pointing, rightly, to the strong sense of economic deprivation among the Barotse people.

Political behavior in an emergent or new African state must be seen in multi-variable terms rather than in terms of a single variable—here, the factor of tradition. It is for this reason that recent writers on Zambia, such as Rotberg and Molteno, prefer the term "sectionalism" to "tribalism." They argue that sectionalism is a political phenomenon, arising from the competition for power among leaders who make use of linguistic, regional, and other social distinctions to create sectionalist identifications as the basis for recruiting political support among people anxious to secure access to economic resources. If this interpretation is accepted, we can conclude that in the modern period, the Lozi, like others of Zambia's sectional groups, were competing for the economic resources made available through the sale of copper, and that this competition was often played out within the idiom of tribalism.

Dr. Caplan has produced a useful and readable book, for which he has drawn on Lozi oral tradition as well as European documentary sources. But his failure to define his terms, notably nationalism and tribalism, leads him sometimes to draw rather doubtful conclusions from the facts which he marshals with considerable skill.

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Technology and the Economic Development of the Tropical African Frontier. By Thomas R. DeGregori. (Cleveland and London: The Press of Case Western Reserve University, 1969. Pp. 539. $15.00.)

Technology, Tradition, and the State in Africa. By Jack Goody. (London, Ibadan, Accra: Oxford University Press on behalf of the International African Institute, 1971. Pp. 88. £1.75.)

These two books together take a historical perspective on a relatively neglected field—the social functions and political implications of technology in Africa. Jack Goody's book is a short but important work that relates political anthropology to technological change in Africa; Thomas DeGregori's book is a much longer exercise in political economy, again in the context of the history of technology.

Both books take us through early phases of political and economic organization in Africa's history, the growth of statehood, a growing awareness of the potentialities of certain resources, and the consequences of discovering new means of exploiting those resources. DeGregori is particularly concerned with development and the sophistication of the means of production. The distinctiveness of Goody's approach is that he sees technology in Africa's history not only in terms of its effect on the means of production but also in terms of its implications for the means of *destruction*. Even the emergence of the metal spear was a significant variable in the process of consolidating monarchical systems of government in Africa. The use of metal was a leap towards statehood to some extent, at least as compared with a combat culture based on the bow and arrow. In the words of Jack Goody:

> "The bow and arrow is essentially a democratic weapon; every man knows how to construct one; the materials are readily available, the techniques uncomplicated, the missiles easy to replace. . . . With the technologies of the bow and stone-tipped arrow, any kind of centralization is almost impossible. But with the introduction of metals, kingdoms are on the cards" (pp. 43–6).

According to Goody, the uneven distribution of raw materials involved systems of exchange and opened up long-distance trade, which in turn necessitated systems of control and security. Furthermore, the manufacture of metal weapons became complicated and specialized, and those equipped with the necessary skills acquired a new level of access to power. Thus, we are told, in some parts of West Africa, special kin-guilds of blacksmiths transmit their traditions among their members; and in centralized groups like the Mossi "the guilds often have a special relationship with royalty, who are often their major patrons" (p. 46).

With the subsequent coming of the rifle in colonial Africa and finally the tank in independent Africa, military skills became more specialized and hence more confined to elites. The old days of military democracy—when everyone passed through the warrior stage and the simple weapons could be made by the warrior himself—were now replaced by the era of professional military specialists, with weapons requiring high technological skill to manufacture and some specialized training to use. The effect of this new military technology on domestic power relations in Africa has been enormous. The history of the military coups on the continent in the last seven years is in part a history of the impact of military technology on the power structure of the new African states.

DeGregori's book, on the whole less original than Goody's, is packed with much more information and bibliographical guidance. DeGregori's book is perhaps longer than it should have been, but it has definite advantages in the immense accumulation of both data and suggestive insights concerning this particular aspect of the political and economic consequences of technological change in Africa. DeGregori is especially good in discussing the relation of *values* to technological response. He takes us through the defiance of the Négritude school of African political thought—the sort of defiance which made Aimé Césaire cry out:

> Hurray for those who never invented anything
> for those who never explored anything
> for those who never conquered anything
> hurray for joy
> hurray for love
> hurray for the pain of incarnate tears!
>
> ....
>
> My Négritude is no tower and no cathedral
> It dives into the red flesh of the soil.

(This translation from the French is borrowed from Gerald Moore, *Seven African Writers,* 1962, p. viii.)

There is in Négritude an element of romantic primitivism. DeGregori does not hesitate to preach to Africans. And why should he hesitate? He tells us that:

> Africans need not consider borrowing technology a mark of inferiority. All cultures throughout their development borrow from other cultures unless they are isolated. It is precisely this isolation, or the lack of opportunity to borrow from other cultures, that is the major cause of technological backwardness, not race or personality. African cultures have shown themselves to be adaptive in borrowing technology from others (pp. 65–6).

DeGregori is aware, however, that technology is not a value-free phenomenon, but carries within itself a set of normative presuppositions which the author calls "technological values." He refers, for example, to the simple value of punctuality.

Less explicitly DeGregori talks to us about the effect of urbanization in Africa upon the speed of technological acculturation. In England in the eighteenth century, the migration from the countryside to the cities turned out to be irreversible. The enclosure movement of the previous four decades at that time helped to sever the link which many people might have retained with their ancestral countryside. But in Africa the persistence of migration of nomadic laborers—men who were peasants at one time of the year and industrial workers at another—has had its own consequences for technological change. To some extent technological acculturation may have been slowed down by the persistent ruralism of African population working in cities. On the other hand, the movement from urban to rural may be a slow but significant force in the diffusion of technological values from the industrial plant in the mine of Lusaka or the factory of Kinshasa to the villages to which these urban workers periodically return. DeGregori relates a frontier theory to expanding technological horizons and economic goals. Again, his notion of a moving cultural frontier is the most stimulating in this section of the book.

Goody is less concerned with applying old concepts to new situations, and more concerned with evaluating the relevance of systemic models derived from Europe for African history and political evolution. He pays particular attention to the concept of "feudalism" as abstracted from European experience, and as applied to oriental and later African state structures.

Both Goody and DeGregori are prone to moralize; they address their preaching to scholars generally and to those who live in Africa. DeGregori advises Africans not to be ashamed of borrowing new technology from others (e.g., p. 66); Goody advises contemporary scholars not to be ashamed of learning "from the studies of medieval historians" (p. 15). Goody is the more subtle and probably more disciplined; DeGregori has obviously acquired massive scholarly material and seems keen to share as much of it as possible with other scholars. Such a predisposition is a value, but it can lead—as often in this case—to a clutter of facts and quotations from others, inadequately integrated. DeGregori's book would have benefited from the application of such "technological values" as organization, economy, and discipline.

But having said all that, we are still left with a persistent admiration for the work both Goody and DeGregori have done as pioneers in the new subfield of "the politics of technological change."

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Catholic Radicals in Brazil. By Emanuel de Kadt. (London: Oxford University Press, 1970. Pp. 304. $9.75.)

This is a case study of the Brazilian organization MEB (Movement For Basic Education), a church-sponsored, government-financed program which since early 1961 has been active in rural areas of Brazil's less developed states. Operating both through radio transmission, assisted by community monitors, and training sessions conducted by visiting teams, MEB has emphasized literacy and other basic education, self or community help training, and what might be called political socialization and mobilization—all at different times in the organization's history and in different parts of the country.

The book's title excites the reader's curiosity about the nature of radicalism and its connection with the church in Brazil. The answers lie in the origins of several organizations. As de Kadt recounts in Chapters 4 and 5, one in particular—University Catholic Youth (JUC)—underwent a period of great internal strife during the mid-1950s. The conflict focused on the desire by militants for greater activism and posturing on sociopolitical issues versus the more conservative position espoused by some members of the church hierarchy. Opposition by that hierarchy and the intellectual preoccupation of the main body of JUC with other issues caused activists to seek another vehicle for social action. AP (People's Action) was founded by the most militant JUC's along with others, outside of Catholic student circles; ultimately, MEB's leadership cadres were drawn from AP.

The *weltanschauung* of AP (chapter 5), de Kadt tells us, emphasized full human development, first through the elimination of the structures of capitalism, but more important, through individual and group self-awareness, understanding, and choice. The author considers this movement as basically populist, and he contrasts it with "massification," which literally means being politically manipulated as part of a mass. AP's analysis of Brazilian society employed the conceptual pair of the dominated pole and the dominant pole, derived dialectically from the history of domination, as per Marx and Hegel, applied to Brazilian social history. Industrialists, financiers, and land owners, with overlapping interests were believed to make up the dominant pole; communism, the AP declared, had merely substituted the state as the dominant pole and had failed to allow plural participation. This epistemology created a problem for MEB's cadres—namely, how do those of the dominated pole develop awareness of their condition and thereby the ability to seek their own interests without manipulation by MEB change agents? The idea of *Concientização* (consciousness raising) as the process of developing awareness of social and economic conditions emerged (Chapter 6). *Concientizaçao* was conducted as part of literacy training using lessons which describe the existential situation of the poor such as their economic condition and treatment by land owners. Also, gradually, through discussions, students were to become aware of their environment's potentialities.

As de Kadt makes clear, the Catholic church in Brazil by no means takes monolithic stands on sociopolitical issues, and so long as the activists of MEB did not penetrate the zone of indifference of conservative bishops or threaten the security of the church, the program was able to develop independently, though not without controversy. But several events which culminated in the 1964 military takeover of Brazil drastically affected MEB. One of these events was the issuance in 1964 of the book *Viver é Lutar* (*To Live Means to Struggle*) as the basic textbook of literacy training and *Concientização*. The ensuing criticism of the book and the interposition of the coup caused the bishops to replace the primer with a book less radical in format, and to exercise greater control over MEB. Emphasis also shifted to literacy and religious training.

The author presents some vignettes of MEB operations in several regions of Brazil (Chapters 9, 12, 13) which demonstrate the variety of conditions among the rural poor. These differences, he says, caused MEB to respond differently to local conditions, in some cases, for example, advocating rural syndicalism, as contrasted with an emphasis on individual and community self-help. Frustration often occurred when the commitment to nondirective self-determination obliged MEB cadres to accept fully what was decided by the people. In many cases no decisions were made.

The author concludes his analysis with three general hypotheses about populism (page 267 ff.): (1) "populism is unlikely to survive when it is harnessed to political mobilization"; (2) there is an inverse relationship between the *effective* purity of a populist movement and the presence of other movements which are not inhibited about manipulating people for their own ends; and (3) the maintenance of populist purity is inversely related to the breadth of scale of the movement. These generalizations are important contributions to the literature on political movements. The book also provides information on Brazilian politics during the Goulart regime, with insights on the radical student movement and attitudes of the Catholic church particularly toward improvement of the life of Brazil's rural poor.

In concluding, I have two critical comments, neither distracting from my overall positive evaluation. First, the author uses the term "populism" in its generic sense to mean a grass-roots movement arising outside of established political channels. In Brazil, however, the concept has been associated more narrowly with what he identifies as "massification," especially with reference to mobilization of urban labor under Vargas and Goulart and its direct control through the Labor Ministry. The author does not reconcile this inconsistency explicitly, but implies that the two concepts are connected because they both produce political socialization (page 36). While this is perhaps true, the theoretical inconsistency remains between the connotation of mass manipulation and that of self-determination.

The second criticism concerns *concientização.* The peasant who has no subtle understanding of his social reality does not suddenly develop this awareness without some relevant experience. In the MEB case, the experience was indoctrination, but one must infer this fact from the author's analysis, since he does not acknowledge it explicitly. This is in spite of the ample evidence he presents in case studies and quotations from *Viver é Lutar.* Perhaps de Kadt's personal identification with MEB caused him to overlook this point. In any case, the issue is an important one, for again it makes explicit the inconsistency between manipulation and self-determination.

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Ulster at the Crossroads. By Terence O'Neill. (New York: Humanities Press, 1969. Pp. 201. $4.50.)

The Price of My Soul. By Bernadette Devlin. (New York: Random House, 1970. Pp. 224. $1.95.)

Ireland Her Own. By T. A. Jackson. (New York: International Publishers, 1971. Pp. 514. $1.95.)

Few participants in the affairs of Northern Ireland are likely to write books, and the field is left largely to the journalists. So the books by Bernadette Devlin, anti-Unionist leader, and Terence O'Neill, former Unionist premier at Stormont, provide useful insights of those engaged in the action. Miss Devlin's book is a mixture of autobiography and personal accounts of her role in the conflict. O'Neill's book is a collection of his speeches with an introduction and running comment by John Cole.

Jackson's *Ireland Her Own,* first printed in 1947 and now reissued with an epilogue by C. D. Greaves, is a conventional recital of nationalist Irish history with an occasional Communist twist. Jackson argues that the Irish nationalists and the English proletariat have a common cause against the English financial power and together can end partition. But the weight of evidence is that political power mobilized in England solves English problems and that Irish alliances with English power have proved expensive. Perhaps he relies upon international socialism to assure a just outcome. He says, "... one looks with confidence for an Irish enthusiasm for the Soviet Union parallel with that of Tone for the revolutionary people of France" (p. 19).

Socialism and Irish Nationalism are equally obnoxious to the Unionist leaders, but they themselves have no adequate answer to the plight of the country. The grace, moderation, and understatement of Terence O'Neill's language in *Ulster at the Crossroads* seem out of place in Belfast, as the premier discovered when he was ousted from office by the local leaders of his own party. For the most part the language was gentle and even timid when he advocated reform, especially before mixed audiences. But the majority of his speeches appear to have been directed to Unionist audiences, and tucked in among the admonitions to reform are echoes of the traditional Orange slogans of "Not an Inch" and "No Surrender." Speaking of reforms he said, "I would suggest ..." Speaking of the maintenance of the British connection he insists, "We must ..." Perhaps, had O'Neill been able to develop a program of social justice ending the old establishments, and had he found words to transcend the local history and sectarianism, he might have rallied the overwhelming majority necessary for reform. But Devlin and Jackson agree that this would have been the path to socialism, a path O'Neill could never take.

Nor have the Unionists appreciated the urgency for drastic change in the pattern of segregation. O'Neill, though dedicated to ending religious strife, saw nothing wrong with denominational organization of social institutions. As a remedy for the difficulties in a dual society he suggested that Catholic and Protestant organizations should associate themselves as "twins" as do the Chambers of Commerce in the paired cities of two foreign lands. Replying to the criticism of gerrymandering he claimed the Stormont Parliament was democratically elected and made no mention of the local councils where the problem was acute.

O'Neill's essential goal was economic progress coupled with toleration. But as Devlin says, he was unwilling to pay the price in surrendered privilege. For economic progress he urged hard work and loyalty on the part of the employees, and attractive living conditions for employers so that skilled and imaginative managers might be lured from Britain. For toleration he urged that Nationalists give allegiance to the constitution and at the same time he asked that Northern Ireland's "peculiar institutions" and folk festivals of parades and vengeful challenges be cherished as a heritage. Bernadette Devlin correctly describes his program as enough to frighten Orangemen and not enough to satisfy their opponents.

It is evident from the events which have transpired since O'Neill's departure from office that no premier of Northern Ireland can bring peace to the province, and the situation is instructive for those interested in federalism and devolution. A local legislature at Stormont allows the Westminster leaders to escape responsibility, and for fifty years the Westminster Parliament has generally ignored the province except to provide a subsidy. Yet the Northern Ireland economy is dependent upon the decisions made in London. Since Stormont deals with few significant issues other than the defense of the regime, which the Unionists insist is not debatable, the authority of the Stormont ministers in the Unionist Party is slight. O'Neill ruefully recognized in one speech Madison's maxim that in such small political communities special interests may exercise great power. The real political power rested with business leaders, local party leaders, and the Orange Order. O'Neill's speeches reveal the weakness of his position. It is not surprising that O'Neill was overthrown by the local party bosses after being hounded by Ian Paisley and thrown off course by the Queens University students. It is no wonder that Nationalist representatives find attendance in the Parliament pointless and that Bernadette Devlin treats Stormont with contempt.

Indeed, Bernadette Devlin speaks in no muted voice. She said she needed no notes for her maiden speech in Parliament when all she had to do was look into the face of a Unionist member and find in it reminders of all she wanted to say about the tragedy of her country. Her origins were humble and her childhood trying, so this youngest member of Parliament speaks with the voice of experience on at least those matters that concern her. Her socialist direction follows no ideological lines but seeks relief from poverty and unemployment and from bigotry and parochialism. Somewhere beyond, perhaps in the socialist nationalism of James Connolly, she hopes to find peace and economic well-being.

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Apathy and Participation: Mass Politics in Western Societies. By Giuseppe Di Palma. (New York: The Free Press, 1970. Pp. xix, 263. $6.95.)

The nicest thing about this book is that the author is willing to speculate—to risk venturing where strict attention to presumably "hard" empirical data would not permit. About a third of the study is relatively "soft"; it involves the author's judicious and inventive use of historical and contemporary information to explore problems of political participation in modern democracies and complex societies. It includes too a most interesting comparison of structural factors that impinge on participation in the United States and Italy. Without these chapters, the book might well be cited as a limiting case of methodological/empirical overkill. The good that will perhaps derive from the methodologically exhaustive analysis is that the book may become a capstone in the use of data gathered thirteen years ago by Almond and Verba.

In addition to intellectually engaging speculation, the author provides a clear and thoughtful introductory essay on the relationship between nation building, modernization, and participation. Many readers will want to challenge some of his claims about the relationship between class, ideology, and participation. But the introduction is quite important in that it sets the premises and delineates the conceptual bases for the hypotheses to be explored later.

My deepest reservations about Di Palma's study involve some of his conceptual distinctions and the use of indicators derived from the Almond-Verba data for central concepts and variables. For example, I found the basis for distinguishing dissatisfaction from disaffection to be very weak. Equally questionable are the indexes from which degrees of dissatisfaction or disaffection or both are derived. Whatever may be the quality of the data gathered by Almond and Verba, it is probably dubious procedure to base major indexes (that then become independent variables) on as few as two or three items in the Almond-Verba questionnaire. If my reservations here make sense, the problem is quite serious, since one of Di Palma's major efforts is to show that it is disaffection and not merely (and *never* directly) dissatisfaction that leads to withdrawal and nonparticipation.

I was also struck by how difficult it is for writers to adhere rigorously to one and only one empirical referent for a key concept. In this case the concept is participation, the major dependent variable in the study. It is clear enough that when the author arrays data and comments on statistical relationships he means by "participation" all of the items that go into his "political participation scale." It is less clear, however, whether he means more than voting when he discusses other writers who have dealt with participation or when he is writing more generally about the impact of, say, education or social status on participation.

It is possible that the cursory reader of the analytical chapters will conclude that many of Di Palma's findings are already well known, given *The Civic Culture* and, say, the astronomical number of seminar papers produced by graduate students with access to the Almond-Verba data set. We surely already knew that Italy is less participant than Germany, the U.S., and the U.K.; that participation increases as one's sense of efficacy increases; that the more "pride-in-nation" one expresses, the higher his participation score; that education is positively related to participation, and so on. What Di Palma does, however, is considerable analysis in depth to give us much more refined propositions in this area. Thus, to cite one example, the strongest statement to make about sense of efficacy and participation is that a lack of the former results in almost total actual nonparticipation. Or, to take variables like income and education, Di Palma persuasively shows that income is stable across the four nations as an independent variable and further that its impact on participation is negligible when education and occupation are controlled. Furthermore, he demonstrates very well indeed the extent to which factors like education and occupation weigh *differently* on participation as one moves from one country to another.

This latter finding provides an important bridge or transition from the more aridly empirical to the more speculatively empirical and normative concerns of this volume. For one of the most arresting suggestions Di Palma offers is that a cumulation of structural conditions, including life chances, not only influences political participation but creates highly stable, self-perpetuating subcultures of nonparticipation. Thus, in the cases of both Italy and the United States, poverty alone will not account for disaffection from and nonparticipation in the polity. Historically rooted cognitive systems and a middle-class definition and style of politics may very well contribute to deeply institutionalized patterns of nonparticipation. Similarly, political processes dominated by middle-class definitions, styles, and values will tend to diminish certain kinds of political opposition and to underscore the meaninglessness of certain forms of participation such as voting. Although Di Palma may not intend it, his study provides considerable reinforcement for the critics of democratic pluralist theory, for members of the New Left and the so-called "counter-culture," and for all of those, from Peter Drucker to Herbert Marcuse, who have underscored the essentially spurious quality of voting as a genuinely efficacious political act in a highly industrialized society.

Without providing any detailed guide to what might be, Di Palma stresses the need to search out new forms of participation. He joins a growing number of writers who lean in the direction of both decentralized and corporativistic modes of participation. He is concerned that contemporary complex societies cannot meet conditions of democracy and of democratic opposition delineated by generations of democratic theorists. He is equally concerned that archaic forms of participation when insistently propounded and demanded in the name of the collectivity may enslave rather than liberate the individual. Evoking a value that can easily be obscured in our race for scientific elegance, Di Palma says, "The ultimate aim is not the polity; it is man and his ability to develop fully his best qualities." There is much in this book that demonstrates both the limits and the promise of political science in helping societies to realize that ideal.

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Resolving Conflict in Africa: The Fermeda Workshop. Edited by Leonard W. Doob. (New Haven: Yale University Press, 1970. Pp. 209. $7.50.)

The study of conflict and conflict resolution is the concern of numerous social scientists. Surprisingly few, however, have dared to subject their proposals to the acid test by applying them to an intense dispute such as can occur between nations. The present volume reports such an attempt and must be regarded as an important landmark in social science intervention in public policy. Doob and his colleagues describe a workshop which they conducted at a

South Tyrolean hotel during August, 1969, in hopes of finding a solution to the border disputes affecting Ethiopia, Kenya, and Somalia. At the heart of the conflict are pieces of land occupied primarily by Somalis, and thus claimed by Somalia, but comprising parts of the nations of Kenya and Ethiopia. For a decade there have been intermittent clashes—and at times armed conflict—between the Somalis residing in these areas and the police or soldiers of Kenya and Ethiopia.

The organizers of this unique encounter were interested in whether sensitivity training could be used as a method for facilitating the resolution of international conflict. Could participants "in the detached and permissive atmosphere of a workshop . . . express and then modify some of their attitudes and values, improve their communication skills, reveal the deeper psychological or emotional issues in the disputes, and thus move closer to an innovative resolution" (p. ix)? Although the African participants were not policy makers of the three countries involved in the disputes and did not officially represent their governments, they were thought to have some informal influence within their governments or at least some access to the men in power. The eighteen African participants (six from each of the three countries) were divided into two training groups (T-groups). The T-groups spent the first week of the workshop learning how to function in an unstructured environment without formal leadership or agenda. During the second week the T-groups discussed the border disputes within the same unstructured environment. Mixed with the meetings of the T-groups were meetings of all participants in a General Assembly.

In an attempt to get as accurate a record of the workshop as possible without using recording equipment, Doob has collected a series of statements from all the types of persons involved in the workshop. There are chapters by three African participants (one from each country), several chapters by the trainers (leaders in the sensitivity training), and several chapters by the organizers (Doob and his associates—William J. Foltz, a political scientist, and Robert B. Stevens, a lawyer). In his preface Doob notes that each contributor was told to write about the workshop as he experienced it. Thus, the reader learns about the Fermeda Workshop—both its successes and failures—from multiple perspectives. To Doob's credit as editor, little redundancy occurs among the contributions. In many respects this book serves as a manual for others wishing to experiment with sensitivity training as a means of resolving conflict in an international setting. Particularly salient to any future attempts is the chapter by Richard Walton, one of the trainers, who thoroughly discusses the basic issues in designing mediation efforts such as those used in the Fermeda experiment.

Based on the written report of the Fermeda Workshop, does sensitivity training appear useful as a means of resolving international conflicts? The answer is equivocal. The participants failed to agree on a proposal for resolving the conflicts among the three countries. Although each of the two T-groups, which included representatives of all parties, arrived at a possible solution, the participants were unable to unify the two proposals. Thus, the workshop ended on a note of dissent. The participant-contributors to the book (an Ethiopian philosopher, a Kenyan political scientist, and a Somalian lawyer and journalist) suggest that sensitivity training is more useful for becoming personally acquainted with the so-called enemy and learning about his attitudes and feelings than for resolving conflict. As the Somalian contributor states: "The personal respect and trust established in the workshop may eventually have direct political consequences. The participants were men of some standing in their countries and could conceivably come to the top. . . . The personal links of comprehension could then be invaluable" (pp. 55–56).

The trainers and organizers remain ambivalent about the success of the workshop. In the last chapter, the organizers indicate their conviction that the conflict could be resolved through the establishment of a Joint Administration Area for the regions under dispute. Both trainers and organizers, however, like the African participants, view their primary success as the substantive interaction of individuals closely identified with governments manifesting intense hostility toward one another—the dialogue among these individuals occurring in a permissive enough environment to promote the probing of an issue important to the conduct of their nations. In actuality the ultimate success of the workshop in resolving the conflict remains uncertain. Several questions require answers. Have the participants discussed the workshop with those in their governments responsible for foreign policy? Have the foreign policy organizations in the three countries considered the alternative solutions proposed in the workshop? Are the governments now in power disposed to settle the conflict—in other words, how salient is the issue to them?

Perhaps instead of perceiving the technique used at the Fermeda Workshop as a means of resolving international conflict, one should view it as an adjunct to more formal types of negotiation. Once parties commit themselves to seeking a solution to a conflict, sensitivity training could be used to provide the environment conducive to creative solutions. An experience of the Fermeda type could occur during that phase of negotiation when possible solutions were actively being sought.

This book serves as a useful companion volume to John W. Burton's *Conflict and Communication: The Use of Controlled Communication in International Relations* (London: Macmillan, 1969) which describes the work of the Centre for the Analysis of Conflict in the University of London. The Centre has employed "controlled communication" in two workshops that have had official government representatives as participants and that have focused on international disputes involving military operations (e.g., Cyprus). Both of these volumes chronicle the beginning of the application of sociopsychological techniques to international conflict. Whether or not the conflicts have yet been resolved, these first halting attempts to apply such techniques to foreign policy disputes add a new tool to man's inventory for conflict management.

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History in Geographic Perspective: The Other France. By Edward W. Fox. (New York: W. W. Norton, 1971. Pp. 190. $6.95.)

The central thesis of this book is that in medieval and early modern France up to the middle of the nineteenth century, two different types of economy and social structure were produced, one in areas that were accessible only to land transportation and one in those that had access to water. The high cost of land transportation meant that although the financial accounts of an area might be balanced in Paris by money taxes and money public expenditure, the physical accounts, the supply of goods in trade, had to be balanced in a small area around a small market town; Fox calls this "subsistence" agriculture. The physical accounts of seaport cities (Bordeaux, Marseille, Nantes) and major river ports (Paris, Lyon, and Rhine cities) could be balanced from distant plantations of the Atlantic, North, and Baltic coasts.

This implied that in the interior there were basically two possible forms of economy—a feudal economy in which the local financial, taxation, and rental accounts were balanced locally under the control of the local nobility or provincial bourgeoisie, and a royal or imperial economy in which taxes and rents went to Paris and were redistributed by the civil service back to the areas where the physical goods were available. The conflict between these two alternatives for the territorial state shaped the political conflicts of the central government with the nobility or the local rich of provincial cities for rural and small-town France. But in contrast in port cities, the central economic relations are free bargains with people elsewhere under a different government, in an Atlantic community, rather than—as in rural areas—authoritative exactions of rents, seigneurial dues, and royal taxes. Therefore the natural government of such cities is a negotiating oligarchy of merchants, of Whig parliamentarians. The feudal versus monarchical conflict of the interior, and the natural alliance of the king or Bonapartist emperor with the peasantry against the exactions of the nobility, do not touch their interests. They go their own Atlantic way except for resistance to mercantilism and an occasional binge of constitution writing.

There are two questions about this: whether the geographical variation is right, and whether it is convincingly connected to varying political behavior. Geologically, Northwestern Europe is divided into three main base rock formations. The limestone bottoms of ancient flat shallow seas occur in two subvarieties, the heavy clayey plains with fertile basic soils of the Seine, Loire, and Aquitaine basins in France, the Southeast of England, the Low Countries, and Denmark, and second the folded limestone hills pushed up around the Alps and the Central Massif, wet and infertile in the North (Jura) and dry and barren on the Mediterranean coasts of Provence. The clayey ancient sea bottom plains are laced with slow meandering navigable rivers; canals and roads are easy to build; the high fertility means that each mile of waterway or road gives access to a lot of surplus food. By the eighteenth century the quantity of food accessible was increasing by the spread of intensive Flemish farming with much use of manure from stall-fed animals, mineral fertilizer, and crop rotation, giving a yield of 10 or 12 bushels of harvest per bushel of seed, in contrast to 4 or 5 to 1 for the three field fallow system. This new system was conquering the plain by about 10 miles a decade as it progressed from Flanders toward Paris.

The second geological formation type is the ancient crystalline rock massif, of Brittany, the Massif Central, the Vosges-Schwarzwald cut by the Rhine, and Wales and Scotland. These are much eroded, with the cultivable alluvial soils being relatively sandy, acid, and infertile. These soils are found more frequently as the rivers that rise in the massif flatten out and slow down to enter the plains around the Massif Central and the Vosges, or the sea around Brittany. Throughout these areas cultivation has apparently always been much more intensive in the river valleys such as the Rhine and the Rhone, and on small delta areas where non-navigable rivers come down to the sea.

The third geological formation is new alpine mountains, wet on the Northern side of the Alps and the Atlantic side of the Pyrenees, and dry and barren on the Mediterranean side of both, except for small pockets of alluvial soils at river mouths. These areas were not very fertile nor heavily populated, and were mostly administered lightly and feudally by the crown as more or less autonomous *pays d'état.*

From this geographical situation it appears that most of the fertile land, most of the intensive agriculture, and most of the people must have been concentrated on plains laced with navigable rivers or along the coasts. I would guess that between half and two thirds of the denser plain populations of the Seine, Loire, and Aquitaine basins and in the Rhineland must have lived within three miles of navigable water. In the Massif Central, Brittany, the Alps, and Provence-Languedoc, I would guess that between a third and a half were in river valleys and deltas or along the coast. We find that even in Limousin in the central massif, Turgot as Intendent arranged to have grain bought in the Low Countries and shipped up the river to relieve a famine. The foreign trade of France in the eighteenth century seems to have increased about ninefold; it is very unlikely that this could have taken place without increasing supply of goods from the hinterlands along the river courses.

This means that the French peasantry mostly *could* participate in commerce. That is, the geographical situation of most of the French peasantry, when closely analyzed, does not support Fox's allegation that rural physical accounts had to be balanced in a small area. Maggiolo's data on literacy in the eighteenth century shows 60 per cent or more with literacy enough to sign the marriage register among males all across the Seine and Loire basins, and more than 35 per cent down the Rhine, near Marseille, and in the Aquitaine basin near Bordeaux. If literacy reflects involvement with urban culture, this supports the idea that peasants in the rich and populous North were extensively involved in their daily life with the city. The density of the network of royal roads in the Northern plain, with sparse roads in the Massif Central, Brittany, and the Midi, supports the same notion. The canal building of the seventeenth and eighteenth centuries suggests that somebody was moving things around in, and between, the Seine and Loire basins.

In sum, Fox's assertion about the subsistence, noncommercial character of most French agriculture by the 18th century is at least not proved, and it looks unlikely for those areas with the most peasants and on which the core of royal administration was based, the jurisdiction of the Paris *parlement* in the Seine and Loire basins. If I am right that there was much commercialization, there should be high correlations between grain prices at various points throughout the Seine and Loire basins with grain prices in the great international markets in the Low Countries, but little correlation of all these with grain prices at, say, Grenoble. If Fox is right, there should be marked regional variation in the direction of price movements even on the plains. Louise Tilly's analysis of the correlations of price movements (*Journal of Interdisciplinary History,* 2, No. 1 [Summer, 1971], pp. 35–45) supports the idea of extensive development of the national market by the eighteenth century.

Now to the politics. The major specific connection between geography and politics offered seems to be that the constitutionalists in the Revolution were called Girondins after the estuary of the Garonne serving Bordeaux. The rest of the analysis is pretty straightforward elaboration of the Tocqueville-de Jouvenel argument of the growth of the state putting the nobles first out of work, and eventually out of power, with the added Marxist speculation that the peasants liked strong kings and loved Bonaparte. It was a brilliant speculation 120 years ago; the fact that it is still a speculation says bad things about the historian's craft. The analysis is a modified version of that form of Marxist history which narrates political events and actions of governments and factions, but simply calls the actors by the names of social classes. Fox calls the actors by the names of social classes and geographical types of economy.

What is needed to demonstrate the connection between geography and politics is the extension backward of Andre Siegfried's brilliant, detailed studies of how political behavior varies with geographical situation, carried out in the Brittany massif in contrast to the surrounding plain, and in the Ardeche river valley versus mountain. We have a first-rate example of what is needed in Charles Tilly's *The Vendée.* Gilbert Shapiro and his co-workers have promised us for many years an analysis of the grievances of the *cahiers de doléances* by the social and economic character of the areas from which they come—an analysis which should settle the question for the last years of the old regime. Fox's general postulate of the conservative character of the peasantry seems to me not proved by election studies for the modern period; I imagine there was a lot of variation in the old regime as well. All of these examples show that connecting geography to politics by way of geographically determined economic and social structure is a lot harder than Fox thinks it is.

My overall evaluation is that Fox has set himself a fascinating task. But he has failed both halves of it, by not showing that the peasantry of France was mostly commercially isolated, and by not showing that people from different geographically determined economies behaved differently in politics.

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Guerrillas in History. By Lewis Gann (Stanford: Hoover Institute Press, 1971. Pp. vii, 99. $3.95.)

There is little evidence that there has been any slackening of academic interest in the guerrilla or his opponents. Historians, participants, organizational theorists, political scientists, journalists, all sorts and conditions continue to add to the literature each in his own way and each apparently without at all diminishing the challenge of the guerrilla. The guerrilla has been approached by a variety of academic means from systems analysis to hagiographic description, for a variety of purposes, many beyond academic confines, and still there is not even general agreement on definitions. Guerrilla theories and insurrection models abound, often convincing none but the converted. And over the whole investigation of the guerrilla hangs the threat of contamination—research bought or encouraged for immediate application, research to patina over a revolutionary commitment, research that is, in effect, part of the guerrilla complex rather than comment on it.

Lewis H. Gann, well known for his work on imperialism and Central Africa, in his brief *Guerrillas in History* is almost completely divorced not only from the potential application of his investigation but also from most of the analytical arguments. He has expanded an article in the *Military Review* into a brief, chronological survey of guerrilla tactics from the Maccabees to Indo-China. There are only the most limited conclusions and those hedged with qualification—no theory, no new methodology, no novel definitions nor analytical insights. For those interested in lessons to apply in the field or political ideas concerning the nature of guerrilla-insurrection—or simply novelty—*Guerrillas in History* will offer little. Yet the monography, a sip of cool water in a flood of vitriolic analysis, has certain simple virtues. In less than a hundred pages, Gann has given a balanced survey of the long history of guerrilla tactics. In any such undertaking, the odd quibble of detail will arise; but Gann's book is sound, touches most of the crucial areas with a sure hand, displays no quirks or hobby-horses, and includes some novel examples—Matabele tactics, or the Anglo-Norman campaign against the Welsh. His few, tentative generalizations are difficult to dispute: partisan operations have severe limitations without contact with a regular force; partisan war exacts a grim price. Ever mindful of the particular, Gann is scrupulously careful to limit his conclusions, "Mass support is vital; yet masses on their own can never win" (p. 87). The burden of the book, however, is briefly detailed in his preface.

> Guerrilla warfare has acquired an aura of romance that once used to be reserved for the exploits of pioneers in the Wild West. The very fashions of many urban intellectuals—berets, boots, and all—have been influenced by the real or supposed examples of Cuban partisans. Yet guerrilla warfare is neither romantic nor new, nor has it always served a revolutionary cause. Neither is it a certain road to victory (p. vii).

Gann's conclusions, or assumptions, are by no means accepted as truisms by many guerrilla theorists and most guerrillas in the field. For them Gann is writing about tactics divorced from strategy, about limited reverses in powerful and certain historical directions. For the more academic and theoretical of mind, Gann's book may in fact be true but on such a particularistic, episodic level that his contribution to the understanding of the guerrilla is negligible. Be that as it may, Gann has written a tidy pocket history of guerrilla tactics, suggested a few conclusions of limited scope and, as a historian, eschewed theory and polemic. Few within the academic guerrilla-establishment can claim as much.

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Les socialistes et la guerre: La S.F.I.O. et la politique étrangère française entre les deux guerres mondiales. By Richard Gombin. (The Hague: Mouton & Co., 1970. Pp. 271. D.G. 25.)

Contrary to the popular assumption that an outside threat compels cohesion, Richard Gombin demonstrates that in the late 1930s external dangers generated by the fascist dictators accentuated differences within the French Socialist Party (then known as the S.F.I.O.) instead of inducing unity. General agreement on foreign policy issues in 1920 permitted the leadership to synthesize differences, but twenty years later the S.F.I.O. was in a state of virtual schism, split between the advocates of concessions (the *pacifistes*) and those of firmness (the *bellicistes*).

From its origins, according to Gombin, the S.F.I.O. "carried the germ of its future decomposition" (p. 256) in the incompatability of doctrinal pacifism with national defense. Although the Socialist Party was able to maintain a facade of unity during most of the interwar years, the fundamental questions of national defense in an increasingly threatening international environment finally disclosed irreconcilable positions. Therefore, "it is points of doctrine which explain the final collapse" (p. 256) of the S.F.I.O., the differing interpretations and policy preferences in the face of war emanating from ambiguities inherent in the Party's ideological foundations.

The author's analysis is primarily based upon the writings and speeches of Party leaders, gleaned from Party newspapers and records of the Chamber of Deputies and Party Congresses. As Gombin notes, many documents concerning the S.F.I.O. disappeared during World War II, and little has been done with what remains. Consequently, Gombin has made a significant contribution to our knowledge of the S.F.I.O's intraparty disputes over foreign policy in the late 'thirties by bringing this material together.

Gombin organizes his study of the foreign policy of the S.F.I.O. around three themes and deals with each chronologically: the problem of war debts, involving a quarrel with the government over means not ends, for Party and government agreed about the need for reparations but not about how reparations should be made; the organization of national security, considered by the S.F.I.O. to necessitate support of the League of Nations and other international peacekeeping agreements; and the march toward war, which marched the S.F.I.O. toward collapse.

During the decade of the 'twenties, the façade of party unity cloaked party differences, but in the 'thirties successive League failures and growing pressures from the fascist states increasingly augmented divergencies within the S.F.I.O., and its followers aligned with either Party spokesmen such as Léon Blum now calling for firmness or those urging concessions to counter the fascist threat. In detailing each disruptive juncture of these troubled years, Gombin objectively presents the arguments of the contesting Party factions. Ethiopia, the remilitarization of the Rhine, the Spanish Civil War—to name a few of the events discussed—all resulted in doctrinal disputes which led the S.F.I.O. to the 1940 rupture. Should Socialists cooperate with a bourgeois government in order to defend France? Should democracy be made safe in France and the revolution left to an unspecified future? Should Socialists fight their own bourgeoisie and take advantage of the situation to take power by direct action in collaboration with the Communists? Should there be a general strike to immobilize the war machine? Both *belliciste* and *pacifiste* found ideological justification in their commitment to peace and socialism, but their antagonistic interpretations ultimately sundered the Party.

A general familiarity with the S.F.I.O.'s organization, history, and domestic doctrines are assumed by the author, and the subtitle well describes the book's subject matter: the S.F.I.O. and French foreign policy between the two world wars. By concentrating on factional differences as revealed by his sources, Gombin generally neglects considerations of domestic political pressures on foreign policy and ignores personalities. Would it not help us better to understand factionalism in the S.F.I.O. if we knew something more about Paul Faure, Jean Zyromski, and Marceau Pivert? Indeed, the reader gains the impression of an intellectual contest somehow removed from men and the strife of French political reality (with the exception of the discussion concerning the intervention-nonintervention dispute during the Spanish Civil War). In short, politics is markedly absent in this study of a political party.

It must be said, however, that Gombin's three aims largely divorce him from considerations other than the purely doctrinal: to set forth and analyze the official party positions on foreign policy; to define the attitudes and preferences of the factions within the party in order to present the entire "physiognomy" of the party; and to explain how positions taken on foreign policy were motivated by ideological factors. The author himself observes that "Especially valuable to us would be a study which would attempt to explain how and why different tendencies evolved" (p. 151). Gombin admittedly does not provide the "how" or the "why," but he has presented a solid view of the "what" of S.F.I.O. factionalism in foreign policy within the framework of his preoccupations. Although frequently commented upon, these divisions have seldom been scrutinized. This descriptive study, however, makes no effort to go beyond the framework in terms of analysis or generalization, as concerns either national or comparative political behavior.

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Guerrilla Movements in Latin America. By Richard Gott. (New York: Doubleday and Company, Inc., 1971. Pp. 626. $10.00.)

In 1967, when Richard Gott was asked to review an English edition of Regis Debray's *Revolution in the Revolution?*, he wrote, "such is our ignorance of Latin America, the Englishmen in a position to query the factual basis upon which (Debray's) theories are erected can be counted on the fingers of one hand." To remedy this situation, Gott wrote *Guerrilla Movements in Latin America*, a 626-page volume, most of which consists of detailed historical, chronologically organized accounts of the five major rural guerrilla movements inspired by the Cuban Revolution—those of Guatemala, Venezuela, Colombia, Peru and Bolivia.

The information Gott presents, which was gathered "mostly from . . . obscure left-wing periodicals, from newspapers (and) occasionally from people and books . . ." (p. 8) does provide the reader with a basis for evaluating several of Debray's major theses. Whereas Debray stressed "the essential unity of the Latin American Revolution" (p. 11), Gott's narrative emphasizes the disunity and incomparability of the various peasant uprisings, officers' revolts, labor strikes and student riots. The failure to date of all attempts to operationalize the *foco* theory of rural guerrilla warfare also raises doubts as to the relevance of the Cuban-inspired model to other Latin American countries. On the other hand, the ephemeral commitment of most of the Latin American Communist Parties to the various guerrilla movements seems to support Debray's critique of these parties.

Data, however, rarely lead inexorably to a given conclusion, and so the conclusions which Gott arrived at on the basis of the information he presents are somewhat different from those noted above. Despite the failure of the attempted alliances between the Latin American Communist Parties and the guerrilla movements, Gott believes that the guerrillas need the Communists ". . . for where else will they find their manpower—and the Communists need the guerrillas—for how else can they maintain their claim to be revolutionaries?" (p. 36). He also believes that the defeat of the guerrilla movements ". . . does not necessarily mean that in other, often-ignored countries" such as Nicaragua, Uruguay or Mexico, "the *foco* theory or a variant of it might not meet with more immediate successes" (p. 484).

Clearly, Gott is more optimistic than I am. He feels that the various reasons he offers for the failure of the guerrilla movements in the five countries he treats—the lack of unity and commitment on the Left, the United States' anti-guerrilla programs, the guerrillas' ignorance of local conditions and the nonrevolutionary nature of most Latin American peasants, all *can* be overcome in the future. The basis for this belief seems to be his conviction that they *should* be overcome. Gott's confusion of fact and value is one of the weaknesses of *Guerrilla Movements in Latin America.*

The book also suffers from an overabundance of detail and insufficient attempts to organize material into general categories of information. As a result, it is difficult to compare the experiences of the five countries discussed. The conclusions presented in the Introduction and the Epilogue, some of which have been mentioned above, are not well integrated with the chronological narratives of the country studies.

Despite its weaknesses, *Guerrilla Movements in Latin America* is superior in some ways to Luis Mercier Vega's *Guerrillas in Latin America* (New York: Frederick A. Praeger, 1969), the other general introduction to the topic available in English. The Mercier Vega book, although more readable and interpretive, provides extremely superficial accounts of guerrilla activities in eight Latin American countries. Gott's effort is more ambitious, thorough, and scholarly. His volume contains much information on topics such as the origins of the guerrilla movements, the background of their leaders, the changing strategies of the various groups, their relationship with the Latin American Communist Parties, the guerrillas' programs and their involvement (or noninvolvement) with the peasants. The eleven appendices of documents, the fifteen-page bibliography listing primary and secondary source material, in addition to the already noted wealth of information, make *Guerrilla Movements in Latin America* a significant contribution which, perhaps, will prove more useful to specialists than to the novices for whom it apparently was written.

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The Emergence of Modern Afghanistan: Politics of Reform and Modernization. By Vartan Gregorian. (Stanford University Press, 1969. Pp. 586. $17.50.)

This book—the best of its kind on Afghanistan—will be a basic source for years to come, particularly since almost 200 of its 586 pages consist of extremely valuable appendixes, notes, bibliography (including pertinent Russian and Persian references), and, finally, an excellent index. The author's competence in all the languages involved in his bibliography is not just impressive, it is probably unique among Afghanists and Iranicists, as well as other specialists interested in Central and South Asian studies and their interrelationships. (Parenthetically, among other things, Gregorian taught me a new English word: eleemosynary, p. 337.)

I shall leave the nit-picking to those who have already nit-picked in other journal review articles, and limit myself to a brief discussion of a few of the book's salient points, taking issue (usually mild to the point of innocuousness) with those interpretations with which, as an anthropologist, I find some latitude for disagreement. For example, the undefined term "feudalism" (which many scholars seem to equate uncritically with the medieval European variety) used throughout troubles me, and I refer the reader to my article (and then the reader will have the privilege of disagreeing with me): "Medieval European Feudalism and the Contemporary Middle East," *Report on Current Research, Spring 1957,* Middle East Institute, Washington, pp. 47–55.

An interesting theme in the book is the implicit equating by most Afghan "modernists" of the introduction of secular *Western* socioeconomic and political models with modernization and progress. Particularly in the light of rekindled and, in my opinion, often positive, ethnocentrism, tribalism, regionalism—or whatever one wishes to call it—in the West, this evaluation may be open to question. Witness the Black Power, Chicano Power, Indian Power movements which, at least partly, arose because the reciprocal, functioning sets of rights and obligations between the government and the governed have broken down—or, in reality, never existed. Crushing the tribal systems *prematurely* can be more disruptive to modernization (however defined) than trying to work within existing regional units until a government can create some sort of reciprocal (and the key word is reciprocal) sets of rights and obligations with the governed.

I believe Gregorian is fundamentally sympathetic with this point of view, but feel that he may have overstated the case against the tribes as being antireform, antiprogressive, and antimodernization. It also seems to me that he plays down the role of those among the urban power elites who have used the modernization processes in attempts to perpetuate that illusory, impossible political institution: the status quo. Gregorian rightly points out that the reform processes have primarily benefited the urban groups. Again, rightly so, because the urban commercial community, easily accessible to revenue officers, has had more than its share extracted from it to support reform and pseudoreform schemes.

The author satisfactorily acquaints the reader with the various social, cultural, economic, and political forces, both cohesive and divisive, which led to the beginnings of modern Afghanistan. He easily handles sources from a variety of disciplines (history, political science, economics, anthropology, sociology, etc.) and knits them together into a meaningful whole.

Having established the background for the action, Gregorian divides the history of modern Afghanistan into seven periods: 1832–1839 (incipient nationalism begins under Dost Mohammad Khan); 1839–1880 (European imperialism replaces Asian imperialism as a major force in the area); 1880–1901 (Amir Abdur Rahman Khan attempts to create a modern nation-state); 1901–1919 (Amir Habibullah continues the processes begun by his father); 1919–1929 (King Amanullah gains political independence, and unsuccessfully attempts to accelerate modernization); 1930–1946 (King Mohammad Nadir Shah and his brothers and son continue modernization at a slower pace); 1946–present ("positive neutralism" and the acceleration of social, economic and political development continues under the leadership of King Mohammad Zahir and his close advisors; the constitutional-parliamentary period begins in 1964).

Gregorian orchestrates all these periods with a masterly touch, ending each chapter with a brief discussion of the achievements and failures of the ruler involved. Throughout runs the leitmotif of the necessity for creating a military base of power to back up reform programs internally and to protect the nation's integrity externally. How effective the military have been in helping bring about sociopolitical reform remains for the future, and I for one, hope there will be a Gregorian in our future to do the analysis.

In the past, all books written about Afghanistan had to be measured alongside Elphinstone's 1815 classic. We now have another yardstick: Gregorian, 1969. Although no one should question Gregorian's scholarship, some of the younger generation of Afghan scholars may begin to question some of his interpretations, particularly in the arena of sociopolitical reforms. But, then, I am sure that is one reason Gregorian wrote the book.

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Neighbors and Networks: The Idiom of Kinship in Social Action among the Ndendeuli of Tanzania. By P. H. Gulliver. (Berkeley, University of California Press, 1971. Pp. xii, 366. $10.00.)

Professor Gulliver's new book is an important addition to the distinguished list of studies in African kinship systems, and is at the same time representative of some recent theoretical developments of profound interest to anthropologists, sociologists, and other social scientists as well.

It concerns the Ndendeuli of Southern Tanzania, numbering somewhat more than 20,000 persons when Gulliver did his fieldwork among them between 1952 and 1954. The Ndendeuli are culturally and historically linked with several of the other Bantu-speaking peoples of this part of the East Africa, and have a relatively simple agricultural economy based upon shifting cultivation, in an area of low overall population density. In an excellently concise historical section, Gulliver traces the recent history of the Ndendeuli through their involvement with the Ngoni and German intruders, the Maji Maji "rebellion," and their subjection to British colonial rule. Their relatively high spatial mobility and obvious hostility to all these "external" influences form an important background to the detailed analysis of their kinship and local organization presented in this book.

As in many other African societies, all important social relationships among the Ndendeuli were expressed "in the idiom of kinship." But, and this lies at the heart of Gulliver's analysis, the Ndendeuli kinship system is emphatically not based upon the principles of unilineal descent so common in much of Africa and the study of which has until recent years been such a major aspect of African sociology. The present study is essentially one of a bilateral kinship system, in which kindreds, kin-sets, action-sets, networks, and clusters provide the elements of the analytical model. In the excellent central chapters on the founding, development, and structure of the local community, the process of dispute settlement, economic cooperation, and intercommunity relationships, a major contribution is made to the study of nonunilineal kinship. The author provides an extremely detailed account of the "closed network" as it is manifested in the social process in three interrelated local communities. The large number of "cases" show the same individuals involved in the shifting alliances of ephemeral action-sets, more stable kin "clusters," and cooperative endeavors, and give a brilliantly clear picture of how this kinship system "actually works."

As far as it goes, his analysis is entirely successful, and amply justifies his claim that the entire range of Ndendeuli economic, political, religious, and neighborhood relations are expressed in the idiom of kinship and encompassed in their bilateral kinship system. "Kinship was essentially an idiom in which relationships were expressed—an idiom easily understood by the people, with an immediacy of its own, and one that had both moral strength and practical utility. Kinship provided a language of communication in agreed terms" (p. 351). And, taking up an issue which has been the focus of long controversy, Gulliver makes clear his conception of the nature of kinship relationships: "Except perhaps in some philosophical sense, kinship does not exist in its own right, but only as a vehicle for other things" (p. 352).

But however important the idiom of kinship and the "kinship ethic," Gulliver found that they were not sufficient in themselves as a basis for action. The two other principles of reciprocity and locality were equally emphasized, and, combined with kinship, produced cooperation. Within the overall "dogma" of kinship, which served "as a simplified symbol for other factors," the principle of reciprocity "was not only a moral principle, though that it definitely was; it was essentially a practical mode of conducting social relations" (pp. 225, 227).

Almost in passing, the book tells us a good deal about a wide range of basic issues illustrated by the Ndendeuli material, such as the critical importance of ecological factors in the analysis of social relations, the role of mediators in the settlement of disputes in systems without courts, the nature of negotiation rather than adjudication in such legal systems, and the related politico-jural basis for dispute settlement, rather than the "judicial." The diligent reader also picks up information on the structure of "quasi groups" and the utility of this concept in the analysis of social action, as well as the manipulative efficacy of influence rather than power wielded by the "big men" or notables in the community. Ndendeuli leaders must avoid even the slightest hint of compulsion and have to rely entirely upon inducing their kin neighbours to support them (p. 251).

It is in the discussion of political relations that some weaknesses appear in Gulliver's analysis. He does cite a case of one notable's enhancing his status in the local community by involvement in wider political issues and economic activities of an entrepreneurial kind (pp. 103–104). But we get no more than a hint of what are obviously hostile attitudes held by Ndendeuli toward the then colonial government, and how these attitudes affected the people's acceptance of "progressive" economic innovations foisted upon them either by the representatives of alien rule or by over-zealous members of their own society whose motives were often obviously suspect (p. 262). It would have been fascinating and theoretically important to have had a systematic analysis of how such a noncentralized, nonsegmentary society adjusted to the problems of external domination and the introduction of alien political roles and structures, and "bureaucratic-type" norms. What does emerge, however, is the intrinsic weakness that a bilateral kinship system such as this one—lacking both politically differentiated roles and corporate groups of a segmentary nature—experiences in handling political relations of an intercommunity, or "external kind" (e.g., pp. 324–327). Further discussion and elaboration of this point would have enhanced the overall analysis.

There is, to my mind, one major flaw which mars this otherwise outstanding book. There is absolutely no systematic discussion of Ndendeuli kinship terminology, nor of the "conventional attitudes" associated with kinship categories. English terms such as "brothers" or "cousins" are frequently used in contexts where their meaning is far from clear, and the cultural norms and attitudes that Ndendeuli must have about at least some of their kinsmen are largely ignored or dismissed as unimportant. Hence, although the "actuality" of the kinship relations "underlying the ideology of kinship" is beautifully demonstrated; the ideology itself is hardly discussed. Gulliver seriously underplays the sociological significance of values, norms, and even emotions about kinship relationships in any society, and too frequently dismisses them as "vague" or "irrelevant." This omission is even more peculiar since the author is obviously aware of the problem, as demonstrated in his by no means short discussion of how he tried to assess Ndendeuli attitudes and motivations in making the choices they did in situations otherwise so well described.

This analysis of Ndendeuli kinship lacks, therefore, a whole dimension of meaning, a dimension which would have told us much more about what Ndendeuli culture and society are really like. My contention is not merely that Gulliver's account lacks "cultural clothing" or Ndendeuli "atmosphere"; it goes to a much deeper, theoretical level. Apart from the development of a network model of social relations, to which this book is obviously an outstanding contribution, it does not sufficiently treat the interaction between culture and society, currently a most significant growing-point in anthropological theory. Where there is some discussion of Ndendeuli concepts, such as that of "the owner of the land" (pp. 63–65) or the very short chapter on "Kinship and Cooperation" (pp. 216–239), the presentation suddenly takes on an added luster and depth, and I cannot help wishing more material of this kind were included throughout the book.

I have emphasized these relatively minor criticisms not to detract in any way from the importance of this outstanding book, but to illustrate the wide range of fundamental issues which it touches upon. It is a fine example of an up-to-date analysis of kinship and a major contribution to East African ethnography in that the analytical framework is thoroughly abreast of some recent developments in theory, even if the data are nearly twenty years old. Professor Gulliver is to be congratulated upon adding yet another volume to his already long list of distinguished monographs in the social anthropology of Eastern Africa.

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Les Communistes en Espagne. By Guy Hermet. (Paris: Armand Colin. 1971. Pp. 216. 34,000 F.)

This highly informative book is by no means easy to read, but it constitutes a mine of reliable information. Despite its brevity, it deals in extraordinary detail with the origins, organization, functions, programs, personalities, and schisms of the Spanish Communist Party since its foundation in 1919 by dissidents from three existing militant organizations: the anarcho-syndicalist CNT, the Socialist Party, and the Socialist Youth, who had organized themselves separately from the official party and were more militant in regard to political tactics. The Communists had to remain clandestine until 1931. They were perpetually plagued by factional struggles, the most significant of which was the refusal of Moscow to permit Andrés Nin to become general secretary in 1927. The party had to accept the Moscow-determined tutelage of Jacques Humbert-Droz during the 1920s and of Palmiro Togliatti in the 1930s.

From 1939 to the present it has again been a clandestine, faction-ridden party.

Hermet gives perhaps the clearest account available anywhere of the tapestry of ideological and leadership conflicts whose most consistent thread is the eventual domination of the party by Dolores Ibarruri (La Pasionaria) and Santiago Carrillo. In its clandestine and factional experience the Spanish Communist party is comparable to the Communist parties of other underdeveloped and dictatorially ruled countries. But during the years 1936–1939 it played a major role in the Spanish Republican government and army, thereby acquiring a degree of power and prestige never attained by any other central or western European party. During the Civil War era it was more a party of bureaucrats, soldiers, and prosperous peasants than it was a party of industrial workers and poor peasants. It publicly defended the bourgeois state and private property against the left Socialist and anarchist collectivist experiments of 1936. It held two cabinet posts, produced some of the most important commanders of the Republican army, and largely controlled the organization and supply of that army.

It should not be surprising then to learn that that party, condemned once again to clandestine operation since 1939, has preached a relatively moderate line. Thus, Hermet tells us, its program for the post-Franco years includes "bourgeois" political liberty, explicit recognition of the role of non-Marxist parties, land reform (but not wholesale collectivization), nationalization of banks, natural resources, and "monopolistic" industries (but maintenance of a large private, non-monopolistic sector), and separation of Church and state (for the mutual benefit of both, but with the retention of Catholic schools and without any anticlerical orchestration). Also, having suffered constant tutelage from Moscow both in the 'twenties and during the Civil War, and having collaborated with bourgeois parties in the government, the Spanish party was not surprisingly more critical than all other European Parties of the Soviet invasion of Czechoslovakia; it is also not surprising that Moscow has retaliated by supporting a splinter faction under the Civil War general Enrique Lister against the official leadership of Santiago Carrillo.

There is of course absolutely no way of knowing how large or how influential within Spain is the present Communist party. Hermet evidently feels that it has very little chance of becoming a legal party again despite its moderate program. Whether Franco is succeeded by a technocratic, conservative monarchy or by another military dictatorship, the Communists will not be invited to participate in political life. And the author believes that the chances for another democratic republic in the proximate future are very slender. The Franquist regime has evolved successful techniques for allowing de facto representation to most of the social elites and class interests of Spain—with the significant exception of the industrial working class. Hermet therefore sees the main function of the Communist party—and also of the left Catholic worker groups—to be the formation of worker elites and the maintenance of proletarian class consciousness and morale.

The author writes with full and sure knowledge of the many ideological cross-currents within the party. For structural and functional analysis he depends heavily on the work of Juan Linz. Only one aspect of the book seems to me unsatisfactory. A reader must surely suspect that behind a great many apparently doctrinal and tactical quarrels lie deep personal power conflicts. Only indirectly can one get a sense of these conflicts, and it is impossible even to estimate their relative importance in the life of an organization whose whole history has been characterized by polemics, schisms, and purges.

GABRIEL JACKSON

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The Economy of Cities. By Jane Jacobs. (Vintage Press, 1969. Pp. 268. $1.95.)

Green-Belt Cities. By Frederic J. Osborn. (Schocken Books, 1969. Pp. 203. $2.45.)

Neither of these paperbacks deals directly with politics, but both have implications for public policy. Mrs. Jacobs, always an interesting writer, likes to think of herself as an iconoclast. Here she seeks to put to rest the "dogma" of agricultural supremacy, or, as she writes (pp. 3–4): "Current theory in many fields . . . assumes that cities are built upon a rural economic base. If my observations and reasoning are correct, the reverse is true: that is, rural economies, including agricultural work, are directly built upon city economies and city work." If this idol has not already been smashed, it certainly has been badly chipped. As the social scientists tell us, a city cannot grow unless its agricultural base can produce a surplus of food and fiber for use by the city folk; but as historians have long argued, human *innovations* have been primarily urban ever since cities first appeared. And Lewis Mumford has argued convincingly that cities predated agriculture in the development of man. Indeed, the theory developed in this book owes a good deal to the earlier work of Mumford, whose name does not even appear in the index. (See his article, "City," in the *International Encyclopedia of the Social Sciences.*)

Mrs. Jacobs develops a detailed theory about how settlements become cities through innovation, with new work spinning off from old. It is a theory of the biological or evolutionary type, cities being alleged to have a period of youth, of maturity, and—in cities like New York today, which have ceased to be centers of discovery and invention—a period of decline. She also explains that "full" employment (or its approximation) can be maintained only if there is a furious rate of innovation, for this is what creates new work. Incidentally, the theory explains poverty as evidence of inadequate innovation. Poverty itself "has no causes. Only prosperity has causes" (p. 121). Mrs. Jacobs discusses why some trading centers become manufacturing centers while others do not, and under what conditions a city which performed one type of function during one period of its existence may perform another major function at another period. And she points out the pattern in the way cities free themselves from dependence upon imports from other cities and develop markets for their own exportation. Like the chain letter, success depends on continuous expansion into new urban markets and into their hinterlands. This is no steady-state theory. (Reading it reinforced my conviction that Japan is the Land of the Immediate Future, for Japan follows the theory in its trade agreements, while the party of the second part often does not.) According to this theory, one should think not of national economies, for there is no such thing, but of agglomerations of urban economies.

The book is written in a pleasantly informal style, with a great wealth of historical illustrations which are by no means limited to Western civilization. And for city-savers in a hurry, the entire theory is diagramed in a nine-page appendix. The book should be of interest not only to those concerned about poverty and "decay" in our cities, but also to those interested in the economies of the "underdeveloped" (i.e., poor) nations. The theory contains important implications concerning the wisdom of much of our foreign economic aid. For politics generally, the most important problem raised is this: Those who control developed economies under any form of government attempt to suppress innovations that may threaten their positions; they have therefore a great incentive to control governments. But if innovation stops, economies stagnate. Can we invent a government that could keep open rather than close off the opportunities for economic and technological development?

Green-Belt Cities was written by Sir Frederic J. Osborn in 1945. This soft-cover reissue includes ten pages of "Prefatory Afterthoughts" and another ten pages of notes designed to bring us up to date on the developments of the last quarter of a century. Osborn, a lifelong advocate of the garden city concept of Ebenezer Howard, wrote a book, *New Towns After the War* (1918), in which he called for the development within the United Kingdom of one hundred garden cities of the size range of 40–50,000 persons. But nothing of the sort happened. This book called for a similar public policy for the years after World War II, and this time Osborn and his supporters had better luck. The New Towns Act, 1946, was passed with all-party support, but the actual development of garden cities was slowed because the Conservatives, always less romantic than Labour, were not greatly interested in a plan that had as its goal "the marriage of town and country." The first six towns were designated immediately and eight more by 1950. During the next decade, only one more town was designated. By 1969, 28 towns were being developed.

Howard's ideas have had some influence in the United States, and a few "green-belt" cities have been developed, both during the Great Depression and in recent years, but these have been primarily middle-class commuter suburbs. In the United Kingdom, in contrast, they have been multiclass cities that are quite self-sufficient, with a manufacturing base. Overall, half of their work force is in manufacturing and the remainder in trade, services, and the professions. There is some commuting, but the principal idea is to allow for urban living under conditions of relatively low density and with a good deal of open green spaces around homes, factories, and businesses.

Osborn describes his theory of town planning and offers a nice history of Howard's two garden cities, Letchworth (begun in 1904) and Welwyn Garden City (begun in 1920). Mistakes have been numerous, he concludes, but perhaps the most common and important one has been the failure to "leave some margin of space for the unforeseen" (p. 10), for technological or other changes that are not anticipated but demand space (e.g., parking lots after World War II).

The garden city has not received much attention in the United States despite its appeal to the American idealization of the county-seat town of two generations ago. Perhaps this is because the commuter suburb has been an adequate and simpler equivalent. Today, concern for all persons irrespective of class or status in an urban environment has been revived partly as a result of a growing awareness that the findings of research in ethology may have important implications for the innate needs of man. The words of Osborn, written more than a quarter of a century ago, may have seemed like romantic sentiment at the time; today they may well suggest a much deeper meaning:

> Howard's conception of a designated country belt around a town answers to an almost eternal sentiment or desire. . . . [He] was the first to unite Plato's conception of the population-limit, [Thomas] More's principle of adjusting by short-distance migration the balance of population in urban areas, and the universal sentiment for a country setting for a town, into a clear formula . . . (p. 180).

Perhaps a rational public policy for man, a creature of the forest and the plains, must begin with an adequate amount of green space, even for those who claim to see themselves purely as urbanites.

Osborn's book has withstood the test of time, though its idealism exceeds its economic feasibility. Jacobs' book may well also withstand the test of time, though its economic soundness may exceed its political feasibility.

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The Economic Impact of the Six-Day War: Israel, The Occupied Territories, Egypt and Jordan. By Eliyahu Kanovsky. (New York: Praeger Publishers, 1970. Pp. xxi, 451. $15.00.)

The economic aspect of the Six-Day War was "either ignored or dealt with rather superficially" by political-military analysts, writes Kanovsky (p. xix). As a major example of the economic myopia which the author ascribes to observers of the political-military scene, he refers to unnamed works that depict the economy of Jordan as "having suffered a fatal blow when Israel occupied the West Bank." On the contrary, according to Kanovsky, "a careful study of the official statistics reveals that main economic sectors [of Jordan] are, in fact, expanding vigorously, and that investment levels are higher than before the war" (p. xix). The penetrating discussion of Jordan's capacity to survive the loss of the West Bank does away with the predictions of doom forecast by United Nations experts and United States officials.

The author's undertaking—to show the economic effect of the war and the economic ineptitude of political-military analysts—is a laudable and valuable venture. The book is divided into sections for each of the three countries and the Occupied Territories. Impact is measured and reported in a straightforward manner that shows changes in conventional economic indices. Each section gives a "picture of the prewar economy, primarily in the 1960's" of the economic system, juxtaposed with much economic data of the postwar period (1967–1969).

Unfortunately, a major shortcoming of the book is that there is extremely little direct cross-country comparison. What, for example, were the *relative* effects of the war on tourism in the different countries? To find out, the reader must search for statistics in different parts of the book. Kanovsky seldom brings the facts together, leaving it up to the reader to make his own comparative analysis. Perhaps this was beyond the author's intended scope; if so, it is a regrettable omission.

On the positive side, Kanovsky provides detailed breakdowns of the prewar economic plans of the different countries—and mentions some of the discrepancies between what was planned and what was achieved. Reference to the various plans leads to expositions of important topics. The Egyptian government's intention to give a bigger role to private enterprise, for example, is shown to have been a response to internal criticism of state enterprises and ineffective bureaucracy. In Israel, the planned cooling-off of the prewar economy resulted in a recession, rising unemployment and deepening national gloom.

Kanovksy's descriptions of the postwar economies include glimpses of such phenomena as Israel's soaring defense expenditures and its acute labor problem. The rising costs of national security are shown to be based on a number of factors: longer borders, the need for more sophisticated equipment, and the escalation of the armaments race. The labor problem is a result of the postwar boom in the construction industry. Kanovsky also points out that the economic orientation of the West Bank toward Israel has been of mutual benefit: Arab workers are imported to help solve Israel's labor problem, while Israeli experts help modernize Arab agriculture (the major sector of the West Bank). This discussion of the territories is valuable because it is new to the literature.

Mr. Kanovksy's work is undoubtedly a useful reference book, in spite of the author's own admission that a substantial amount of information is not readily available or exists in distorted form—especially concerning Egypt. This limitation leads, at times, to superficial anecdotal reportage; Kanovsky does not say whether the absence or distortion of data affects the accuracy of his measurements.

The book leaves the reader hanging; it sorely needs a fifth section to tie the other sections together. Unfortunately Kanovsky has abdicated the responsibility for developing analytic categories that would aid the comparative analysis of the countries and would better indicate the *relative* impact of the Six-Day War on the various countries.

EFRAIM TORGOVNIK

Tel-Aviv University

Fragile Victory: Prince Saionji and the 1930 London Treaty Issue from the Memoirs of Baron Harada Kumao. Translated with an Introduction and Annotations by Thomas Francis Mayer-Oakes. (Detroit: Wayne State University Press, 1968. Pp. 323. $10.00.)

An enduring problem of politics everywhere is the question of who in the government is competent to decide what. The situation in the prewar Japanese government was particularly complicated because the Constitution of 1889, which represented a compromise between Western parliamentary forms and an entrenched oligarchy reluctant to share power, was purposely vague on a number of important points. One of these involved the authority of the government to enter into international agreements which would limit the size of the Navy. Both the Army and Navy had rather early fought to establish the principle that only the armed forces were competent to decide on such matters as military planning, strategy and operations, in short on the whole question of national security.

In 1930 Japan participated in the London Naval Conference which sought to head off an arms race among the great powers by continuing, with some modifications, an allocation of ratios in naval strength agreed upon at the Washington Conference in 1922. At the time of the London Conference the government was headed by Hamaguchi Yuko, a political party leader who was anxious to avoid the escalating costs of armaments in a period of economic depression. Some high naval officers, especially in the Navy General Staff section, argued that the ratio proposed in London would jeopardize national security and challenged the right of the government to enter into an international agreement under those circumstances. In the end the London Treaty was signed and ratified, but only after a long and bitter behind-the-scenes maneuvering within the upper reaches of the government.

Professor Mayer-Oakes's book, *Fragile Victory,* looks at this struggle from the vantage point of Baron Harada Kumao, a political secretary of Prince Saionji, the last of the *Genro* or elder statesmen who advised the Throne on important state matters. Harada functioned as a kind of "leg-man" for the aged and retired Saionji, reporting the latest news and gossip to the Prince, and carrying messages from him to important personages in and out of government. Because he was Saionji's envoy, Harada had access to all leaders; even the Emperor himself phoned him on occasion. Baron Harada had the foresight to dictate once every three or four days his recollections of his conversations with the Prince and the numerous individuals he talked with as he made the rounds in government and court circles. Some years ago the transcript of these notes was published in Japan, and *Fragile Victory* represents a translation of that portion of the Harada material that relates to the London Conference.

Professor Mayer-Oakes has made an excellent translation. The English rendering not only reads smoothly, but it also conveys the spirit of the Japanese text. He has also included an Introduction which examines the origin and background of the Harada papers as well as the political setting in which the London Naval Conference struggle occurred. Finally, copious footnotes identify personages and events mentioned in the text and cite additional relevant works.

Fragile Victory is of obvious interest to students of Far Eastern politics, but in addition it should be useful to those political scientists concerned with the general problem of how different governments arrive at important policy decisions. One will find here all kinds of revealing information. For instance, we learn that both the Navy Chief of Staff and the Vice Chief who were opposed to naval limitation made public statements and leaked reports to the press to buttress their position. The Grand Chamberlain, a retired admiral and one of the three important officials in the Imperial Court, told Harada that he felt that the Navy Chief of Staff should be more discreet and circumspect, and added that "It is highly reprehensible to drum up popular support for his own notions and then to try to push them through because of the public opinion thus aroused" (p. 103). On the other hand, Admiral Okada, whom Harada saw on numerous occasions, acted as a mediator between the pro- and anti-treaty groups. To play this role he had to act as if he favored the "hard line" policy because his role as mediator would have been weakened if he had been tagged as an exponent of the "soft" policy. He told Harada, "I must don the mask of the die-hards and play this in order to effect a settlement." In short, the Harada memoirs give us a good deal of insight into the political culture of Japanese elites and their political style in the 1930s.

NOBUTAKA IKE

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Le Conflit sino-soviétique et l'Europe de l'Est: ses incidences sur les conflits soviéto-polonais et soviéto-roumain. Jacques Lévesque. Preface by François Fejtö. (Montréal: Presses de l'Université de Montréal, 1970. Pp. xiii, 387. $9.85.)

Since the very beginning of recorded international politics, it has been a truism that the fate of small states is decided by the vicissitudes of great power conflicts. It is equally true, however, that the minor states have always quickly learned how to manipulate conflicts among the great powers to their own advantage. A major conflict remains, of course, a source of sparks, real or verbal, and consequently attracts most of the attention, publicity, and also scholarly research; but significant and lasting shifts on the international scene are often rather the result of maneuvers and realignments on the part of small powers than of the big powers' direct actions toward each other.

The Sino-Soviet conflict is a classical example of a great power confrontation, and it continues to be studied primarily in these terms, with the focus of attention on the two main protagonists. The overwhelming emphasis on the great power aspect, and consequences, of this conflict is perhaps better justified in this case than in many others. The country with by far the largest territory in the world confronts the country with by far the largest population; between them runs the longest common boundary in the world. If one adds the role which adjoining India, the power with the second largest population in the world, played in this confrontation both politically and militarily, one cannot help noting that never in world history did so few states involve in a conflict such a large share of the world's territory and population, all of it contiguous. The conflict might well have assumed a quite different character, more in line with that predicted by Harrison Salisbury in his book *War Between Russia and China,* had it not been that other geopolitical factors counteracted the impact of those already mentioned, specifically, had it not been for two factors: (1) the Sino-Indian and Sino-Soviet boundaries were able to protect sovereign "impenetrability" because of immutable physical geographical factors rather than from manipulable political and military might, and (2) the demographic, cultural, and economic centers of all three powers were asymmetrical in relation to the centers of national territory and located in opposite directions, far from the foci of territorial disputes.

The territorial contiguity of the protagonists, while emphasizing and acerbating the great power nature of the conflict, has unavoidably relegated to a distant plane any role which the small powers could play in it. With the sole exception of Mongolia, the small powers were peripherally located, on the other side of China and the Soviet Union respectively, and their role remained peripheral. The very location having precluded any pivotal geopolitical or geostrategical role for their territories, the small powers (or satellites of the communist world as they used to be called) would, perhaps, neither have been drawn, nor permitted to step, into the conflict at all had it not been for the ideological differences involved. Starting from the ideological plane the issues were then quickly extended to a host of political and economical questions.

Lévesque's study is the first systematic, competent book-length treatment of developments in some of the small communist countries as related to the Sino-Soviet conflict. Not surprisingly he chooses to deal with some of the so-called satellites from the western, not the eastern, end of the Eurasian land-mass under communist control. It may be a long time before a political scientist considers he has enough information and documentation to write an equally extensive study on the role of the Sino-Soviet conflict in the politics of North Korea or North Vietnam. It is also not surprising that Lévesque singles out for attention Poland and Romania, which are much more open to this kind of inquiry than, let us say, East Germany or Bulgaria; nor that he focuses on Poland in the period 1956–1959, the more or less "open" era of Polish politics, and on Romania in the period 1960–1968 when Bucharest became increasingly bold, admitting in private that it actually did manipulate the great power conflict. The subtitle of the book is a more accurate indication of the contents than the main title: The center of the stage is occupied by Poland and Romania, while the treatment of Eastern Europe as a whole is merely sketched in as background information. One should add that throughout the book the emphasis is on the impact of the Sino-Soviet conflict on the politics of Eastern Europe. There is little about the reverse side of the medallion, and in this case again it was undoubtedly the relative availability of reliable information that was decisive. What impact the politics of the East European and Far Eastern communist countries may have had on Peking and Moscow may remain a secret to western scholars for a long time.

In the first part of the book the author deals with Poland and in the second with Romania, and he draws many interesting parallels and contrasts. He naturally emphasizes the role played by nationalism and quite correctly sees it as a constant factor which merely surfaces or submerges according to circumstances and the mood of the people. In the case of Romania, he points out that factional and personal struggles within the Party strongly influenced attitudes towards the Sino-Soviet conflict. In general, the author seems more at home when analyzing the Romanian than the Polish scene, but throughout he provides an informed, sometimes original analysis. He probably considerably exaggerates the impact of the Sino-Soviet conflict on Poland and Romania, even though he does not, like some analysts, ascribe most of the freedom of action gained by Romania, for instance, to a skillful exploitation of the clash among the rivals for leadership in the Communist World. In my opinion, the author has not sufficiently emphasized that by and large the Sino-Soviet conflict was not welcomed in Eastern Europe as a God-given opportunity for expanding elbow room; on the contrary, it was feared as a threat to the slowly expanding freedom during the era of *détente* between East and West. Tensions among the great powers of the world, whether belonging to different or similar ideological camps, were invariably accompanied by (1) Soviet attempts, mostly successful, to tighten their grip over the actions of the governments and parties of the East European countries and, (2) a tightening of these governments' and parties' grip over the domestic affairs and the freedom of action of individual citizens.

On the debit side of Lévesque's book one must note a surprisingly meager bibliography. This may be due to the author's lack of command of either Polish or Romanian. He does claim, however, to read Russian, yet not a single Soviet book or article is listed in his bibliography. One must also note the absence of a subject index; there is a name index, but what good is a name with a string of as many as sixty or seventy page references listed after it, without further indication of the specific subject matter to which they refer?

Ladis K. D. Kristof

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Colonial Rule and Political Development in Tanzania. By Gus J. Liebenow. (Evanston: Northwestern University Press, 1971. Pp. 360. $11.50.)

Political scientists scarcely began to take an interest in Africa until the colonial era was almost over. They witnessed, recorded, and analyzed the final development of African nationalistic opposition to colonial regimes and the emergence of the new states. Thereafter, that is during the nineteen-sixties, political scientists have very largely concerned themselves with matters and problems relating to centralized, state government: new constitutions, political parties, bureaucracies, dictatorship, political coups, the role of the military or the trade unions, relations with East and West, African socialism, the politics and administration of development, and so on. All this has, of course, been both necessary and useful, even if somewhat lacking in theoretical sophistication and too much focused on political historiography. For each Apter there have been a score or more of fairly worthy journeymen. There has been a general recognition of the problems of ethnic disunity, regional diversity, and weak or nonexistent national identity, in these artificially created, new states; and from the central viewpoint a few valuable analyses of these factors have emerged. But we know really rather little of what has been happening at the local level and the regional level, and what is happening now in the first decade of independence of most of the African states. How far have indigenous political institutions, roles, values, survived? How, if at all, have they affected modernization processes in the twentieth century? How have other cultural factors, economic development, local chauvinism, affected political development? How have local areas, or distinctive regions, fitted into the new centralized regimes? Who have been the individuals, and what their roles, who have hindered or facilitated political change and efficient administration? Who have benefited most, and whose vested interests have been trampled on?

These kinds of questions are important in any radically changing country, but they are especially crucial to African countries because of their internal diversity and the absence of common tradition and consensus. And the fact is that we can scarcely do more than guess at the answers or make generalized, often unsubstantiated, assessments. Unfortunately the anthropologists, who might have been expected to provide some of the data and answers, have been altogether too concerned with reconstructing and analyzing "traditional" political systems of local African societies.

Dr. Liebenow deliberately concentrates in this new volume on one ethnic group in southern Tanzania: the Makonde, about 300,000 people, and the third largest ethnic group in that country. In essence he has written a political history of the Makonde from the late nineteenth century to 1968. The descriptive content is marked, but throughout the author has attempted serious analysis and interpretation. He has accomplished something which we need for many, many parts of diverse Africa, but the like of which exists for only one or two local ethnic groups.

Dr. Liebenow rightly begins with an account of precolonial political organization among the noncentralized Makonde, with few specialized political institutions and roles. Perhaps with anthropological prejudice, this reviewer finds this part of the book least effective and comprehensive, verging sometimes on the superficial. And this defect dogs some of the later account where, it seems, insufficient account is taken of Makonde cultural bases as the people reacted and adapted to the successive phases of colonialism and of independent African government. It is clearly not enough, and not good enough, for example, to refer several times to "Makonde apathy" to innovation and opportunity. This shorthand expression hides more than it reveals; it evades some of the basic issues orienting Makonde behavior and defining issues and interests as Makonde perceived them.

The treatment of the early stages of colonialism, German and British, is much better, despite unavoidable scarcity of hard data; and the analysis of the introduction, failure, and replacement of "indirect rule" by the British is excellent. There is also a neat illustration of the "white man's madness" (a Makonde term, but one instantly recognizable by anyone who has experienced British colonial regimes) by which local policies, programs, and emphases, changed, often quite radically, with each of the frequent changes of colonial administrator. Dr. Liebenow has done the earlier and middle colonial period well, despite some inadequacies of material; although he tends to assume that the written records left by colonial administrators (in the District Book and elsewhere) are more trustworthy than some of us would be prepared to accept.

The account of the late colonial period and on into independence starts admirably with the identification and description of five categories of Makonde: parochial traditionalists, tribal nationalists, accommodating modernists, dissenting modernists, and pivotal modernists. Like all such categories, they are not watertight ones; but their delineation is most valuable, and it would be interesting to see if comparable categories occurred elsewhere, and not only in Tanzania. Thereafter, Dr. Liebenow examines the gradual (really rather speedy) triumph of Julius Nyerere's nationalist party, and the early years of independence up to about the time of the Arusha Declaration. It is a difficult task to keep a balance between the focus on the local, the Makonde, whilst setting that in the wider national scene; and perhaps the author has not been invariably successful. One sympathizes with his problems, and as a pioneer in African micropolitical study he has succeeded in so much. It is to be hoped that he and others will follow up this study, learn from its undoubted merits as well as its deficiencies. One must applaud not only its intention and program, but also the generally high standard of its implementation. It is an important book, and just as much for those who could not care less what happened in this one corner of Africa.

P. H. GULLIVER

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The Politics of Formosan Nationalism. By Douglas Mendel. (Berkeley: University of California Press, 1970. Pp. 260. $7.95.)

Professor Mendel's book invites two criticisms, one concerning its aim and the other its methods. As for his purpose, the author tries hard in the introduction and first three chapters ("The Growth of Formosan Nationalism," "Revulsion Against Early Nationalist Rule," and "Formosan Response to Cultural Integration") to establish his hypothesis that Taiwanese are not Chinese and Taiwan is not an integral part of China. Arguing that the overwhelming majority of Taiwanese abhor both the Nationalist and Communist regimes, Mendel consequently avers that Taiwanese separatism is justified and that the silent majority on the island would endorse secession even if it cannot say so now. After examining all evidence and facts cited by the author, any knowledgeable reader would find it impossible to accept the author's definitive but paralogical hypothesis. While Mendel contends that the legal status of Taiwan remains unsettled and that the

1952 Sino-Japanese Peace Treaty merely stressed Japanese renunciation of Taiwan and the Pescadores without naming a beneficiary state (pp. 174, 181–185), he fails to inform his readers of the legal significance of Article VI of the Sino-American Mutual Defense Treaty clarifying that "for the purposes of Articles II and V, the terms 'territorial' and 'territories' shall mean in respect of the Republic of China, Taiwan and the Pescadores . . ."; he also ignores Article IV in the 1952 Sino-Japanese Peace Treaty recognizing that "all treaties, conventions and agreements concluded before December 9, 1941, between China and Japan have been null and void as a consequence of the war." The latter provision will suffice to repudiate Mendel's contention that China has never extended sovereignty over Taiwan and clearly reaffirms a historical fact that Taiwan was taken away from China by Japan under the Sino-Japanese Shimonoseki Treaty of 1895, not from an independent Taiwanese regime. Besides, the author's justification of the Taiwanese secession movement rests mainly on a specious assumption: Formosans' resistance of cultural integration through "persistence of Japanese heritage" and constant use of local dialect.

To say that Taiwanese are not Chinese is as inaccurate as to say that Cantonese or Fukienese are not Chinese. It would be fatuous and impractical to insist on imposing on China the internationally approved independence of Taiwan. Any serious student of such matters would realize that neither the Chinese Government in Taiwan nor the Chinese Communist regime in Peking has the mandate or the sovereign power to preside over an international liguidation of China's territorial integrity. No one could realistically expect that any new Chinese regime could be dictated or goaded into accepting such an outlandish solution, even if one assumed that both the Nationalists and the Communists would give in or vanish. Mendel's support of the Taiwanese secession movement at the expense of China's territorial integrity cannot escape the charge that it is simply a replay of the defunct international scheme of partitioning China. Under the guise of nationalism, the author would equally not object to having Manchuria, Sinkiang, Tibet and Mongolia break away from China. Those who follow Professor Mendel's advice might ruefully find that such a course could lead eventually to a fratricidal war.

Furthermore, the author has mixed objective facts with value judgments. The lack of a conceptual framework for his research has meant that the impressive quantity of his information does not lead to any credible or sound conclusions. My own reservations about the author's techniques of observation and method of analysis are threefold: (1) The omission of a bibliography is inexplicable, as is the exclusion of important documents. The sole appended document on "Declaration of Formosans" can hardly be considered objective or sufficient. (2) The selection of the categories of documents consulted seems arbitrary and defective. For example, evaluative categories covering the standpoints, values, and authorities are often based on the foreign information prejudicial to China's legal right to Taiwan. The author has used both indirect extensive observation and direct intensive observation. In the extensive observation Mendel has relied more heavily on the quota method than on the probability method of sampling, and has drawn too heavily on Japanese and American polls. (Out of the eight sources, four are Japanese polls and one is an American poll.) As for the probability method, his choice of the individuals interviewed is also subjective and his choice of strata is similarly unsystematic. For example, the author did not even bother to poll the Chinese residing in the United States, Japan, and other areas. Unfortunately the margin of error in these unrepresentative and limited interviews cannot be measured. (3) The author is not sufficiently meticulous about the statistics he cites. For example, says the author, "there were almost 500 junior high schools at the end of the Japanese occupation, nearly half of whose 16,000 students were Formosans and about 30 higher commercial and normal schools" (p. 48). But according to official Chinese statistics, there were only 41 secondary schools with a total of 29,005 students, and only 12,826 students were Taiwanese (see *Taiwan kuang-fu erh-shih-nien* by Taiwan Provincial Information Service, 1965, chapter 19, pp. 26–27.) Professor Mendel also said: "In 1963, only 13 per cent of Taiwan's total government revenues went for formal education, compared with about 50 per cent for defense . . ." (p. 48). While blaming the national government for spending less than 4 per cent of its budget for education, the author chooses to ignore the fact that the provincial government actually spent an average of 25 per cent of its budget for education annually in compliance with the minimum constitutional requirements.

Professor Mendel should be commended for his commitment to "freedom for all peoples," and his intention "to maintain a certain detachment" so that he can fulfil his duty as a political scientist "to make his research available to the public as well as to governments and other scholars" (p. 4). Indeed, the book would have added some interesting polemics and informative articulation to the whole issue, if the author had exercised some academic continence by limiting his role as a research scholar. But unfortunately Professor Mendel changes his role from research scholar to revolutionary mentor, offering paternalistic advice for prodding the Formosan activists abroad to more militant, programmatic, and revolutionary actions to realize their goals (pp. 242–245). Regretfully, I believe his book performs a disservice to the betterment of Sino-American friendship.

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Public Opinion in Occupied Germany. Edited by Anna J. Merritt and Richard L. Merritt, with a Foreword by Frederick W. Williams. (Urbana: University of Illinois Press, 1970. Pp. xxiii, 328. $7.95.)

According to Frederick Williams, who wrote the foreword to this book about German attitudes in the immediate postwar period, the military authorities in the American zone of occupation were

> centrally concerned with change—change interpreted as the political maturing of the German people, an increase in their readiness to accept responsibility as individuals and as a great nation, a deepening in awareness of the nature of a free society, with its strengths and weaknesses, an improvement in the peoples' knowledge of the history of their own nation and the character of the tyrants they had supported (p. xx).

In order to detect the extent to which change was occurring, the Opinion Survey Section of the Office of Military Government, U.S. (OMGUS) was established and charged with the responsibility of examining German attitudes on a wide variety of issues and problems. For the most part, this book is a summary of the 194 reports issued by this office in the period between 1945 and 1949.

The OMGUS surveys summarized in this book are based on two kinds of samples. On the one hand, there were samples of the entire German population living in the American zone of occupation (AMZON). Overall, these were probably quite representative since they were based on the list of rationcard holders. Initially, these samples were rather small, but by December, 1945, the number of respondents had grown to 1,000 individuals, and by April, 1947, some 3,000 Germans were being questioned by the OMGUS interviewers. On the other hand, there were also samples of specialized segments of the AMZON population. Included in the book are reports based on samples of German elites, German youth, former prisoners of war who had attended a course on democracy at Fort Getty, the residents of Vienna, and 188 people interviewed on the streets of Berlin.

Some of the information provided by the OMGUS reports, such as that dealing with German attitudes toward daylight saving time, will be of limited interest to political scientists. But the abundant data on the political attitudes of Germans and their attitudes towards the occupation period should be of considerable interest to German scholars. Fortunately, the reader interested in gaining an overall impression of the state of German opinion in the immediate postwar period will not have to peruse each of the 194 summaries. The Merritts have provided a balanced introduction in which they summarize the major trends in the reports. The first section of this introduction deals with the attitudes of Germans toward "Immediate Occupation Policies." Among the topics covered in this section are attitudes towards the occupation period, American forces, reconstruction, food allotments, expellees, the licensed press and other media, the economic situation and currency reform, and the division of Germany. In the second section of the introduction, the authors examine the data on "Democratizing Postwar Germany." Here they point out that while few Germans had read *Mein Kampf*, while few trusted Hitler up to the end of the war, and while most were satisfied with the outcome of the Nuremberg trials, nevertheless "large numbers of postwar Germans in the areas under American control continued to express perceptions characteristic of National Socialist ideology" (p. 31). They also note that although the principle of denazification was endorsed by the AMZON population, the implementation of the principle was not. With reference to political participation, the authors report that it was never high during the occupation and that it remained at a relatively constant level throughout the entire period. This latter finding, they suggest, "raises serious question about the overall effectiveness of the democratization program pursued by American occupation authorities—at least in terms of its measureable effects" (p. 50).

The final section of the introduction, "From Democratization to Anticommunism: The Reorientation of American Policy," is by far the most interesting. Here the authors argue that though the conditions were present, the allies did not bring about substantial cultural change during the postwar period. The Merritts contend that the reason the allies failed in this regard is that they allowed the Cold War to get in the way of the goals they were trying to accomplish in occupied Germany. American policy, according to the Merritts, changed in the latter years of the occupation: "It became less interested in creating a new German society than in establishing Germany as a bulwark against communism" (p. 52). This change had serious consequences for the goal of cultural change in postwar Germany. "Promised education for democracy, the Germans ended up getting pushed off on another ideological crusade. . . . Promised new democratic leadership, they got a reentrenchment of leaders from the discredited Weimar period, together with those too clever or lucky to avoid entanglement in the webs of either Nazism or denazification" (pp. 57–8).

This book is unlikely to be widely used in the undergraduate classroom, but it will probably become an important reference manual for commentators interested in changes in German public opinion in the postwar period. Using the OMGUS surveys as a starting point, one can, with the help of the large body of public opinion data compiled by such commercial firms as Allensbach and EMNID, trace the development of German attitudes toward a variety of topics and issues across a period of more than two decades. It should be noted, however, that the OMGUS surveys are confined, for the most part, to Germans living in the American zone of occupation, and that the punched card data are no longer available. Thus, individuals interested in the attitudes of Germans in the immediate postwar period will either have to be satisfied with the summaries provided in this book or will have to wade through the 2,000 pages of the original reports available through the United States National Archives and Records Service.

This book does not contain a great deal of penetrating or controversial analysis. Indeed, it seems unlikely that anyone would sit down and read it from cover to cover unless there were some compelling purpose—such as being asked to review it! Yet, the book contains much useful information, and those interested in understanding the postwar development of German political attitudes will find it valuable. For reading the reports and compiling the summaries, therefore, the Merritts are to be congratulated.

Kendall L. Baker

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Barrios in Arms: Revolution in Santo Domingo. By José Moreno. (Pittsburgh: University of Pittsburgh Press, 1970. Pp. 226. $8.95.)

Vietnam and Santo Domingo; Santo Domingo and Vietnam!—the two great U.S. interventions and foreign policy crises of the mid-1960s continue still to haunt us. Obviously the far larger war and quagmire of Southeast Asia have overshadowed the smaller and now out-of-the-headlines conflict in the Dominican Republic; but one may well argue that it was the Dominican revolution and the U.S. response to it that undermined the confidence of opinion leaders in this country toward Lyndon Johnson, that presaged the crisis of credibility that pervasively plagues us, that destroyed forever the position and posture of the U.S. as a peacekeeping nation dedicated abroad as well as at home to the expansion of democracy and social justice, and that helped usher in the decline in our faith and sense of meaning and national purpose. Moreover, even though it now seldom makes the front pages, Santo Domingo—since the aborted social revolution that the U.S. snuffed out—has continued to disintegrate into virtually perpetual conflict and violence; it testifies to an incompleted revolution that has left a bitter legacy of hate, fear, bloodshed, and simmering anti-Americanism, as well as a political system that cannot cope with the uncontrolable forces pushing it toward breakdown let alone begin to solve pressing national problelms—and much of the responsibility must be laid at the doorstep of the U.S. and its ill-conceived policies.

Initially our miscomprehension of the Dominican revolt was augmented by the coverage it received—journalistic, largely sensationalist and ill-informed, rabidly Marxist and economic determinist from one point of view, properly liberal and indignant from another, and a third perspective officially sanctioned or commissioned by the Johnson administration. Now, however, with Jerome Slater's 1970 book and Abraham Lowenthal's forthcoming study, the Dominican revolution and the intervention are beginning to receive the detailed, scholarly treatment they merit. To this distinguished group we must now add José Moreno's *Barrios in Arms*.

José Moreno was a Cuban-born Jesuit priest doing doctoral dissertation research in Santo Domingo on structural anomie in formal organizations at the time the revolt broke out on April 24, 1965. Immediately caught up in the revolution and generally sympathetic to its broad goals, Moreno found the original project impossible to research and soon dropped it. During the first days and weeks of the revolution he helped organize food dispensaries and medical services within the rebel zone and became a participant observer of the revolutionary process. Later, realizing that he was the only trained social scientist in the constitutionalist area (as well as virtually the only priest), Moreno began to study the revolution more systematically, drawing upon his earlier observations as well as the published sources, various private documents, and extensive depth interviews. The result is a book which, as would be expected given the circumstances, is sometimes uneven and ambiguous methodologically, but utterly fascinating as the first and probably only inside view by a professional sociologist of the dynamics of the revolutionary movement.

Moreno is careful to catalogue the mixed motives of many of the revolution's leaders, the diversity of leadership after some of the democratic-left elements took refuge in foreign embassies, the differing ideological perspectives and private ambitions that governed the revolution's participants. He also describes more explicitly than anyone has previously the important role the several Communist groups *did* play in the revolution (as distinct from the U.S. Embassy's infamous "lists" of 55 or 76 Communists that were demolished by James Goodsell's investigative reporting). But what Moreno also makes abundantly clear is the widespread opposition on the part of the Dominicans to the ousted Reid Cabral regime, the desire of the general population to cleanse the country of Trujilloism, of military corruption and oppression, and of widespread in-group favoritism and profiteering, and in so doing to establish a new order based on principles of democracy and social justice. Even with the qualifications that Moreno registers, the inescapable conclusion is that the revolution and its leadership were social-democratic in aims and aspirations, that the Communist influence was never dominant (and could not have become so), and that in essence the revolution represented a genuinely popular uprising of the poor and downtrodden against a corrupt military-oligarchic system that oppressed them and failed to recognize their human dignity. At the same time, the revolt is revealed as a movement with fairly limited goals: to use circumscribed and carefully structured violence to establish the right and legitimacy of the pro-Bosch forces to participate in the national life from which they had been excluded, that is, to secure the place in the system which democratically they were entitled to but without destroying totally the system per se. Misunderstanding this, badly informed, naively and mistakenly seeing in Santo Domingo a repetition of the Cuban experience and blind to other considerations save the "second Cuba complex," the U.S. over-reacted, bottled up and frustrated the revolutionary movement, bolstered the efforts of a discredited and defeated military and oligarchy to regroup, rearm, and re-enrich themselves, and thereby set in motion the destructive, disintegrative processes in which both we and the Dominicans are now caught.

There are many fascinating glimpses as well as considerable imaginative analyses within the Moreno book. His investigation bears out that the democratic-left PRD (the Bosch, constitutionalist party) and its military supporters always remained the dominant force within the revolution, even though the realities of the situation and of their ideology obligated them to consult democratically with other groups. Moreno's treatment of the ideological currents within the revolution, of the emergence of leadership in a revolutionary setting, and of the forces allied on both sides of what became both an intervention and a civil war shows solid scholarship and original treatment. His division of the various groups participating in the revolution into political organizations, paramilitary organizations, and civilian organizations provides a new and intriguing perspective, and his analysis of the latter two is about the only detailed account we have of these groups and their activities.

There are also some flaws in Moreno's analysis. His treatment of the internal dynamics and sociology of the revolutionary movement is solid and informative, but his comments on the international aspects are sometimes weak and incomplete. That, however, is not Moreno's field and in any case is superbly treated in the Slater and Lowenthal studies. Occasionally also the book reads like the resuscitated dissertation at Cornell that it eventually became. And like many sociologists, Moreno sometimes oversimplifies the political complexities and undercurrents he describes. But on balance his remains an immensely worthwhile account, a very nice complement to the literature already available, and a fascinating glimpse into one of the most important but now largely neglected upheavals of our time.

HOWARD J. WIARDA
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The Red and the White: Report from a French Village. By Edgar Morin. Translated by A. M. Sheridan-Smith. (New York: Pantheon Books, 1970. Pp. 263. $7.95.)

Ever since Tocqueville's nineteenth-century recollections of his rural constituency and Halévy's reminiscences of his retreat in the Massif Central, village France has had an irresistible literary appeal to urbanites, French and foreign alike. In contrast to the popular French tradition that regarded the countryside as idyllic, if not bucolic, these writers have stressed the complexity of life in rural France and the great sophistication of the peasant. But paradoxically (and rural France is rich in paradox) the more specialized the study, and the more advanced methodologically, the more disappointing the results. Indeed, all has been downhill since Lawrence Wylie's classic *Village in the Vaucluse,* the least scientific and most profound French village study, appeared in the 1950s.

With the publication in 1967 of Edgar Morin's *Commune en France* and its translation as the *Red and the White: Report from a French Village,* the corner has been turned. A specialized attention to both detail and essence, a combination of historical, economic, political and anthropological perspectives, and a prose style that even translation does not diminish, enable Morin to engage and hold the reader's attention through two-hundred and fifty pages of description of what life is like in *Plodémet,* a rural township far out on the Breton coast.

But can a case study of *one* village take us very far in arriving at valid generalizations—the real basis of all scientific work? The answer is yes, and not because Plodémet is presented as "typical" of all French villages, which it is not. First, while Morin's *genre* is descriptive narrative, the hard skeleton of the study is analytical. It is a book about the roots and the consequences of change in the rural periphery of advanced western societies in the 1960s. In its concentration on the itinerary of change, the study transcends the case study format. Its methods are well adapted to its aims, although the total balance of the interpretations would have been improved by a comparative perspective. For example, Morin shows no knowledge of such classic studies as Vidich and Bensman's *Small Town in Mass Society.*

The reader may well ask if the village case study does not inevitably suffer from the inability of the lone sociologist to "get deep inside" the village community in all its complexity. In this case, several years of data collection and analysis by an assorted team of researchers from several disciplines preceded the monograph. Morin drew fully upon their research in the preparation of this book, and this contributes to the richness of the presentation, if not to its theoretical coherence. For example, while Wylie could only look into the municipal records of his Provençal village, the historical reconstruction of Plodémet and its political development cannot be matched.

What pulls the study together? Morin makes much of an approach he calls "the sociology of the present." This seems to amount to a preference for participant-observation, and is in no sense a full-blown theory or methodology. The somewhat pretentious appendix devoted to this approach can therefore be ignored, and the theory-seeking reader must plunge into the narrative itself to divine the author's epistemology. Here he will find not only one implicit model, but two: Morin was trained as a Marxist-Leninist, and this shows in the criteria he used in selecting his material, particularly in the historical discussion. But he is also a gifted student of culture and has been deeply influenced by the French school of cultural anthropology. Thus, he emphasizes language, mass communications and religion, as independent forces of social change, as well as the push and pull of market forces, the control of the means of production and the clash between bourgeoisie and proletariat.

On the whole, Morin's dual perspective on social change enriches the analysis. Still, the combination of Marxist and cultural anthropological perspectives often produces odd results. For a Marxist to talk of the "personality" of a village practically in the same breath as he writes of "the dynamic forces of history" has an awkward ring. More important, neither approach gives much independent importance to local politics, and we see the local political elite mainly at the receiving end of two lines of change: the economic forces that have transformed the peasantry and the town dwellers, and the cultural changes that have produced a wide gap between the generations. The subtle interplay between local and national French politics, as seen particularly in the dual role of the small-town mayor, receives very little attention in the book.

I must say, in retrospect, that the best chapters are not those which, like "The Emerging Bourgeoisie," bring out Morin's Marxist background, but are those which, like "Women, the Secret Agents of Modernity," and "The Young and the Old," stress most vigorously the cultural aspects of change. (Parenthetically, Morin does much for the cause of Women's Liberation by showing that change in Plodémet is chiefly inspired by women.) He is best in writing about human beings under the stress of changes they only partly understand. His book suggests that towns like Plodémet will never be "small towns in mass society," if only because they are small towns in an *old* society, a society with the cultural resources to traditionalize change and modernize tradition.

SIDNEY TARROW

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Africa Must Unite. By Kwame Nkrumah. (New York: International Publishers, 1970. Pp. xvii, 229. $6.95.)

Kwame Nkrumah has reissued his 1963 call for continental unity unchanged beyond a short author's note to the new edition. In this, he points to the wave of military coups d'état and the increased imperialist and neo-imperialist activity in Africa as proof, in effect, of the truth of his original thesis. Nkrumah's main point is the need for political unity if the African states are to achieve significant and sustained economic growth. His sometimes companion theme—that it is necessary to achieve more than *de jure* political sovereignty—is muted in this book. This is probably fortunate, for his economic arguments are more persuasive without the strident political polemic.

Because the work stands unchanged, we must evaluate it from the perspective of events in Ghana specifically and Africa generally since he wrote it in 1962. Nkrumah was above all an advocate of continental unity and gracefully saw his own stronger draft charter for the O.A.U. bypassed in favor of the diluted, but widely acceptable, version. His vision of a united Africa, and his campaign for that goal, were certainly important in the creation of the O.A.U.

One is surprised that he did not take advantage of his enforced leisure since the February 1966 coup d'état in Ghana to revise much of the book. The first several chapters, which defend C.P.P. policies in the early 1960s, must be somewhat embarrassing to their author in light of the revelations and commissions of inquiry following his deposition. The paragraph on p. 53, for example, on C.P.P. "guidance and support" to liberation movements is, to say the least, whimsical after the discovery of camps to train opposition groups from other independent African states in the arts of violent seizure of power. Similarly, although his chapter on reconstruction and development is academically sound, the injunction to avoid inflation is one that the author did not take note of in his own management of Ghana's economy. In the same vein, Nkrumah's recommendation that small-scale personal enterprise be reserved to Ghanaians is one that awaited implementation from the post-1969 Busia government rather than the author's "Building socialism" regime. One point in his internal policy does remain valid, however. His exposition of the virtues of unitary versus federal systems in the potentially fragmented state seems vindicated by the eventual violent revisions of the two major federal systems, Uganda and Nigeria.

While dropping much of the material on internal domestic politics would have strengthened the re-issued book, the portion on unity and development would have profited from expansion and updating. Nkrumah could have overwhelmed the reader with statistics showing that national growth rates barely matched population increases, or showing the continuing instability in the price of agricultural export items or the still unequal bargaining abilitiies of 'developed' versus 'underdeveloped' nations. A better appraisal of the advantages accruing to African States from affiliation with the European Economic Community would now be possible, had Nkrumah chosen to revise. So, too, would an evaluation of the myriad regional economic cooperative groupings that have come and gone. The East African Common Services Organization has struggled through the 'sixties, but its metamorphosis into a Common Market arrangement has meant a decline in areas of cooperation. Nkrumah's Ghana was one of the first to establish a national (often nonconvertible) currency and others have followed. The likelihood of establishing a common currency, a step strongly urged by the author in 1963, seems more remote than ever. Some halting steps have been taken to improve intracontinental communications—microwave stations now permit direct telecommunication links between some African capitals rather than via Europe, and the proposed East-West road system is moving beyond the talking stage. Nonetheless, the overall economic picture is still bleak.

I particularly regret that Nkrumah did not present his current thoughts on the O.A.U. As a first move toward continental political coordination, if not unity, it was viewed as a significant step in the right direction. Although the O.A.U. has had some successes, its overall record is checkered. The South African regime is more solidly in control than ever (Nkrumah's hopes for a revolution there, pp. 14–15, have not been borne out in the decade since Sharpesville); the regime in Southern Rhodesia has just been legitimated by the United Kingdom; and the liberation movements in Portuguese Africa have achieved significant success only in Guinea Bissau. Nkrumah's own prototype for unity, the Union of African States (Ghana, Guinea, and temporarily Mali), which was to provide the nucleus of a future United States of Africa, has fared rather worse. It's only real accomplishment was to provide a legitimate haven for Nkrumah after the coup d'état. Though Touré greeted him then as "fellow president" of Guinea, Touré has shown no inclination to take advantage of Nkrumah's political expertise in the governing of Guinea.

ROBERTA E. MAPP
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Chilean Politics, 1920–1931: The Honorable Mission of the Armed Forces. By Frederick M. Nunn. (Albuquerque: University of New Mexico Press, 1970. Pp. 219. $10.00.)

This is essentially a history book, and it is problematical whether we should apply modern political science standards to an evaluation of it. It is a detailed chronicling of the role of the Chilean military, particularly the army, in Chilean politics during the most politically unstable decade the country has experienced in this century.

Briefly told, the "Honorable Mission" of the Chilean armed forces was "to set right Chilean politics and to assure governmental responsibility and action in the social and economic fields" (p. 3). In Nunn's view,

> military deliberation of political matters became necessary in the 1920s because civilian leaders proved unable (in some cases unwilling) to carry on the affairs of state; social conditions had become acute. . . . The military was the only organized group in Chile capable of carrying out such action. No civilian interest group, faction, or party was capable (p. 5).

The "Honorable Mission" of the armed forces, therefore, was to purge the Chilean political system of the vices of excessive parliamentarism. The economic stagnation and social malaise of the post-World War I era were laid at the door of civilian politicians and the structure of parliamentary democracy. In Nunn's view, "action by military men put an end to the political stalemate in Chile" (p. 27).

The latter generalization is highly questionable, but this should not obscure the importance of these recurring themes in the study of twentieth-century Chilean politics. The second administration of Carlos Ibanez (1952–58), the Christian Democratic administration of Eduardo Frei, and the recent triumph of Salvador Allende's Popular Unity coalition all have this much in common: They, too, attributed the frustration of Chilean economic and social aspirations to root political causes and they, too, were concerned with altering a situation perceived as one of "stalemate." But Nunn's study of military intervention in the decade of the 1920s is not set in the context of such important theoretical questions concerning Chilean politics.

Students of civil-military relations in developing countries are unlikely to find much material of cross-national relevance in this study. Occasionally, interesting observations are made which bear directly on some of the major topics in the literature of civil-military relations, but again, these are not followed by systematic elaboration. On page 9, for example, Nunn argues that "military professionalism and civilian parliamentarism produced an antagonistic civil-military relationship unheard of in Chile since the 1820s," thereby implying that the growth of a professional military establishment in the late nineteenth century contributed to the military's overt intervention in the civilian political process. Occasional reference is made to "corporate self-interest" as a possible cause of such intervention. In terms of the class bases of the officer corps, we are told that "in the 1920s the overwhelming majority of junior army officers came from a middle class, often provincial, background; most of the generals, however, identified with Chilean conservatism through family ties or political connections" (p. 55). As a summary of the causes of military intervention, we have the following statement:

> The aftermath of the Civil War of 1891—army dissatisfaction with a corrupt, unresponsive government and its supposed favoritism toward the navy, increasingly serious social and economic conditions and mounting desire for a return to executive leadership, and the intra-army schisms—motivated the armed forces politically and brought civil-military relations to the boiling point by 1920 (pp. 11–12).

In the concluding chapter, "The Mission Ends," Nunn offers a few suggestions about those factors which contributed to a "return to the barracks." But on all of these matters—military professionalism and its relationship to military intervention, the bankruptcy of the civilian political process, the military's conception of its own institutional role, the class bases of the officer corps, the relationship of "social and economic conditions" to military intervention, the factors making for an end to overt military involvement in politics—Nunn says little which illuminates the theories of Finer, Huntington, and others about civil-military relations generally or the theories of Johnson, Lieuwen, and others with regard to such relations in Latin America.

A perhaps justifiable reply to this line of criticism would be that to do all these things was not the book's intent. If the book is seen simply as an effort to detail events involving the Chilean military in the decade under consideration and is judged solely on those grounds, then we might come to different conclusions about its usefulness. It is a competent piecing together of the major historical events of a chaotic era in modern Chilean politics. Anyone who has studied Chilean politics knows that the task of "mere description" is no easy task; such study is akin to putting in place the pieces of an elaborate puzzle. Nunn has surveyed an impressive array of historical materials in an effort to piece together an important period in Chilean political history, a period which has had deplorably little coverage. For this reason the book is likely to be widely read by confirmed Chileanists. Its useful bibliography on the period will also enhance its utility for students of Chilean politics.

As a description of a particular period in Chilean political history, then, the book is a competent if somewhat dull undertaking. As an analysis of the events described, it is less than convincing. Specialists on Chilean politics will probably find themselves turning to it for essential historical information, but scholars with a wider comparative or theoretical orientation are unlikely to find much that would make it required reading.

Robert L. Ayres
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Words and Occasions. By Lester B. Pearson. (Toronto: University of Toronto Press; Cambridge: Harvard University Press, 1970. Pp. 296. $10.00.)

It is academic blasphemy to suggest that it could take forty years to rise from Lecturer to full Professor in the hallowed halls of Canadian universities. Yet such was the case with Professor Pearson but with exculpation: by his own admission "... I had taken a long non-academic detour (where tenure was at times very dubious) before achieving this promotion," an ambition he harbored as an academic neophyte. Pearson's detour is described in this four-part anthology of his speeches, transcripts, and photographs as an External Affairs department official; Secretary of State for External Affairs; Liberal party and official Opposition leader; and, finally, as Canada's fourteenth Prime Minister. His career belies the cliché "Nice guys finish last."

The book provides the reader with glimpses of urbane self-mockery and other instances of warm humor through selections which an egocentric statesman would not have included—selections such as his maiden speech in the House of Commons and an interview which noted one of the scandals affecting his government. Although the author alludes to some of his many kudos, his sense of humility keeps him from citing the highest award he achieved, the Nobel Peace Prize. His efforts in the formation of NATO and the delight he expressed to some of his Algoma East constituents was overshadowed by one old-timer's basic political advice (which he quotes): "Well, that was a fine thing you did down there in Washington, Mr. Pearson, signing that there treaty, but it won't help you much around here if you don't give us a new post office."

Pearson's entry into the political arena from the top as Secretary of State for External Affairs was compounded by his lack of a "grassroots" upbringing: a sharp contrast to that of his indefatigable and indomitable opponent, John Diefenbaker. Even though he was a diplomatic artisan *par excellence*, Pearson did not have either sufficient political adroitness or Mackenzie King's "killer instinct" to prevent the scandals which became one of the hallmarks of his stay in power—a stay which rested on a plurality but never a majority of seats in the Commons. In this regard his civil service background was a handicap: "... I confess that I couldn't lose the habit of seeing more than one side of a question. A party leader, should only recognize two colours—white, his; and black, the opposition's" (p. 119). At the 1967 annual banquet of the Canadian Political Science Association, he cited his former Professor's definition of politics as "the skilful use of blunt instruments." Many political scientists and pundits wondered whether the Prime Minister had paid more than lip-service to this definition in view of his government's scandals and other blunders. In this same speech he was candid enough to state that he had "not escaped without some bruised political muscles and even a drop or two of spilled political blood." Notwithstanding bruises and blood, the Pearson government achieved measured progress in social welfare, federal-provincial relations, unification of the armed forces, bilingualism, and a distinct Canadian flag.

Paradoxically, while Pearson lacked political adroitness, nonetheless he was endowed with abundant diplomatic acumen and a keen sense of history. He displayed a perspicacious view of the future when he publicly dealt with the destructive potential of developments in atomic energy before an Ottawa Church group in 1934: similarly his distaste for war and commitment to internationalism led him to believe that "all states will have to abandon some of the attributes of sovereignty in the interest of larger associations of nations" (p. 34). As a protagonist for international action, Pearson's qualities made him a positive middleman at the UN. His efforts over the Palestine issue resulted in the establishment of UNSCOP and approval of its partition plan in 1947. On the brink of a major power cataclysm over Suez, Pearson's UNEF proposal on November 3rd, 1956, was the epitome of positive diplomacy designed to "face-save" the Franco-British intrusion, assuage an aggressive Soviet Union and a shocked and chagrined American government, achieve a cease-fire, and, for the first time in history lead to the creation of a truly international peace force responsible to the Secretary-General and General Assembly. This was a remarkable achievement which made him an outstanding winner of the Nobel laurel.

This book is also a useful guide to students of Canadian foreign policy because it addresses a number of issues in policy formulation during the Pearson years: geography and the North American milieu, the British Commonwealth of Nations, and the strength and resources of a middle power ("Canada and the San Francisco Conference"). In his April 1951 speech he commented on Canadian-American relations ". . . the days of relatively easy and automatic political relations with our neighbour are, I think, over" (p. 107). Canada's position in the world had become more important and the United States was now a superpower. Pearson was a champion of the "quiet diplomacy" precept which Canadian foreign policy practitioners retained despite rumblings of its unpopularity during the 'sixties. His continentalism is brought to the foreground in the controversial speech of January 12th, 1963, which called upon Canada to accept nuclear warheads for North American defense, something that the Kennedy administration clearly wanted.

Independence of thought coupled with responsible action is one of Pearson's strongest characteristics which undoubtedly accounted for his election to the UN General Assembly's presidency in 1952. Nevertheless, such independence did much to annoy Mackenzie King, as Volume IV of King's "Record" has recently revealed. In "Some Principles of Canadian Foreign Policy" (January, 1948), Pearson questioned the King government's timid position on the UN Korean Commission and asked why the King government, "after a somewhat adventurous beginning, backed well away from the implications of our earlier words in the sorry and tragic Palestine business?... Personally, I would have liked in this Palestine issue to have taken a strong independent stand for what we thought was the proper and right solution and to have dismissed the British and Americans..." (p. 71). Dismissal of the British but not the Americans over the Palestine issue was inconceivable under Mackenzie King. Pearson did not regain independence of action until nine months later when he was appointed External Affairs minister and won a seat in the Commons, while Mackenzie King retired.

Metaphorically, *Words and Occasions* is the delectable *hors d'oeuvres* plate before the "Pearson Memoirs" main course. It whets the appetite on a number of important international issues which the Memoirs will elaborate upon in detail. His human touches provide the reader with an important dimension of the author's personality, not to be overlooked in future evaluations of the Pearson Years.

ZACHARIAH KAY

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Libyan Independence and the United Nations: A Case of Planned Decolonization. By Adrian Pelt. (New Haven and London: Yale University Press, 1970. [For the Carnegie Endowment for International Peace.] Pp. xxviii, 1016. $35.00.)

Mr. Pelt's book is a comprehensive presentation of the role performed by the United Nations from 1949 to 1951 in the establishment of a sovereign and independent Libya following Italy's defeat in World War II. The reader gets a panoramic view of the functioning of the World Organization in a unique colonial situation, as painted by former UN Commissioner in Libya, Adrian Pelt. Not only does the author trace the circumstances in which the Big Four Powers, out of mutual disagreement on what to do with the former Italian colonies, turned to the General Assembly of the UN for a determination of the case. He also delves into: the historical background of Italy's colonization of Libya as of 1911 (explaining the evolution of the Libyan nationalist struggle against its western colonizer); the sectional orientation in Tripolitania, Cyrenaica, and Fezzan which delayed the emergence of a united Libya; the attitudes of UN Member States towards the various issues raised by the Libyan case: and finally, the emergence of the federal Libyan structure, under King Idris I, in 1951.

The author deals with his subject from the vantage points of having been involved personally in those historical events, of having access to archives and other material not readily available to other researchers, and of having known or witnessed those who played various roles inside and outside the UN in relation to Libyan decolonization.

His references to confidential (later cleared for his purposes) papers, works translated from Arabic, personal papers (such as the Omar Loutfi papers in Cairo) and reviews and correspondence (such as those with Abdel-Rahman Azzam, the distinguished first Secretary-General of the League of Arab States), all add to the richness and variety of his bibliographical data.

Mr. Pelt's description of the endless UN compromises which led to the adoption of General Assembly resolutions 289 (IV) of 1949 and 387 (V) of 1950 illuminates the nature and functioning of international organizations. His analysis of the functional interaction between sources of authority and responsibility in pre-independent Libya is equally enlightening. In addition to the Commissioner himself, these factors were: the UN Council for Libya, created to advise the Commissioner on assisting the Libyans in working out their constitutional arrangements for an independent Libya; the United Kingdom and France in their capacity as administering Powers in the former Libyan territories; the National Constituent Assembly of Libya (NCAL) which opted on 2 December 1950 for a federal form of government; and the King-designate, Idris al-Sanussi. In clarifying the complex process of nation-building under the aegis of the United Nations, Pelt emerges as the outstanding advocate of "King" and "Federation" in Libya, even though these have both been discredited in Libya during the 1960s. His opponents remain, then and now, the forces that opposed "federation" and called for "unity" among Tripolitania, Cyrenaica and Fezzan, as a means of checking imperialist British and French influences.

Unfortunately, Pelt has no ideological contribution to offer in terms of the new positions adopted by the UN as of 1960. He recounts those experiences of the late 1940s and early '50s as if his thinking about the role of the UN in national liberation had completely frozen at the level of 1951. Nothing in the book links what the UN did in Libya in the early '50s with what the World Organization has been doing since 1960.

Not only does the author completely ignore evolutionary change in the relationship between the UN and colonial peoples, he also implies that the close supervision exercised by the UN in Libya is still a valid approach to nation building in the 1970s. It is unrealistic to expect the present leaders of the African revolutionary movements, who have been accorded full recognition by the UN and the OAU as national spokesmen of quasi governments, to accept the UN's Libyan pattern of supervision and interference-by-advice in their constitutional development.

What seems to be paternalism on the author's part manifests itself in three other important respects. First, he distinguishes between what he calls "Libyan nationalism" and "Arab nationalism," as if they were two distinct and mutually antagonistic forces. The establishment in September 1971 of the Federation of Arab Republics, consisting of Egypt, Libya, and Syria, illustrates the fallacy of this distinction. Second, he regards the Western ideal of democratic government, with constitutional checks and balances, to be the only acceptable form of rule for newly emerging nations. Third, he looks upon the Libyan leadership and people of that period as wards in need of his helping hand. In fact, he states that it would have been preferable to delay independence by one year to allow the Libyans "a better chance of assimilating the new ideas, of studying the problems . . . etc." Pelt's inability to distinguish between the popular urge for freedom in colonial areas and the technicalities of framing constitutions and laws is evident throughout his work.

It is not therefore surprising that Mr. Pelt, as the Commissioner, had found it "difficult... not to overstep the mark between help and pressure." Such paternalistic attitudes towards what the author calls "young" countries ignore the historic realities of Libya's long armed struggle against Italian imperialism, then against Italian fascism. The Libyan war of national liberation, the first African war of this kind in the 20th century, has done a great deal to sharpen Libya's sense of its nationhood over a period of 30 years of conflict. The author's insensitivity toward the impact and meaning of that struggle is illustrated by his disparaging comments on the formation of the National Council for the Liberation of Libya (NCLL) under Bashir Saadawi in 1947.

The book's orientation toward what should be described as an outdated Western approach to the Third World is also apparent in the author's comments on his sources. Pelt constantly throws doubt on his non-Western sources of information, without indicating the reason for his doubts. For example, he relies heavily, in the "historical background" part on the work of the Libyan scholar Al-Tahir Ahmed al-Zawi's *Jihad al-Abtal.* Nevertheless, he prefaces several of his quotations by statements such as "if al-Zawi's version of the events is to be believed." In questioning the credibility of his non-Western sources, the author, by the same token, undermines the credibility of important sections of his work.

In summary, within the covers of this 1000-page study, the reader finds a very lengthy case history of decolonization under the UN in Libya, which abounds in source material for future analysis, but misses the relevance of the study to the issues of decolonization today and to the UN's role in them.

YASSIN EL-AYOUTY
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Modernization and Diversity in Soviet Education. By Jaan Pennar, Ivan I. Bakalo, and George A. F. Bereday. (New York: Frederick A. Praeger, 1971. Pp. 395. $20.00.)

Soviet modernization has long interested western social scientists, and the present work reflects a continuation of that interest. As the title suggests, the authors have specifically looked at the interactions between Soviet education and Soviet modernization. They demonstrate how the changing needs of Soviet economic development have affected the content and structure of the public school system. Conversely, they show how the schools prepare the younger generation for different labor roles and how the educational system has helped shape and sustain rapid economic growth. In analyzing these interdependent phenomena, the authors have assembled and evaluated a large amount of primary data. But most of these data have been analyzed before by such scholars as DeWitt (*Education and Professional Employment in the USSR*) and Osborn (*Soviet Social Policies*). Consequently, the book does not substantially add to or advance our knowledge of the subject.

A more serious flaw stems from the authors' exceedingly narrow treatment of the concept of modernization. They list four goal objects of Soviet educators. We are told that the public schools have been charged with the task of rearing a "new man, one that would be isolated from the decadent tendencies of the old society." A second goal is that of creating a collectivist value orientation so that the school graduate will ask not what his country can do for him, but what he can do for his country. Third, "The Soviet leaders are intent on creating a politically docile man characterized by joyful obedience to the government." And finally, education must produce an industrial man "to prove that Communism is the quickest way to enrichment" (p. 98).

This reviewer would have added other goal objects which the state has assigned to the educational system. Whether there are four, five, or eight socialization goals, however, is not the main point. What is important is how the educational system functions to achieve these goals. Is the classroom experience structured in such a way as to foster the development of a collectivist as opposed to an individualist identity? How does the school curriculum generate political legitimacy and political support for the communist regime? What aspects of the educational program are designed to nurture the personality traits of the New Soviet man? How successful has the educational system been in realizing the goal objects identified by the authors?

These and many other questions have to be answered if we are to understand the correlations between Soviet education and Soviet modernization. Yet the reader finds very little information which might illuminate these issues. After presenting a multidimensional picture of the goals of Soviet educators, the authors choose to concentrate exclusively upon those educational aspects which relate directly to the training of industrial man. Any work which fails to discuss the political, cultural, and social psychological dimensions of modernization suffers not only from imbalance, but also distorts the very phenomenon it sets out to explain.

Along with its narrow focus upon labor training, the book is repetitious. Many events, such as the Educational Reform Act of 1958, are discussed time and again. With proper editing the work could have been shortened substantially without impairing its basic theme or value. As a reference source on Soviet education and economic growth the book has utility. But as an empirical study of comparative modernization, the book badly fails.

JOEL J. SCHWARTZ
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The Politics of New States: A General Analysis with Case Studies from Eastern Asia. Roger Scott, ed. (George Allen and Unwin, 1970. Pp. 201. £2.10.)

This collective enterprise takes the form of a number of essays (completed early in 1969) designed to introduce Australian undergraduates to a growing field of academic enquiry. The blurb on the dust-jacket represents this book as satisfying "the special need for a comprehensive course of study," but the author with becoming modesty views the endeavor as "a selective study." To this end a general introduction explaining the nature and problems of politics in new states is followed by a number of chapters whose prime unifying link is the relevance of the countries to Australian interests.

In many ways this is an unexceptionable book. The editor provides the reader with an elementary but useful survey of the principal characteristics of new states and deals competently in the process with the relevance of conventional concepts, the significance of precolonial influences, the experience of decolonization, the problems of political and national integration and the failure of postindependence political institutions. In all this, there is perhaps a tendency to overgeneralize from African experience—given the geographic orientation of the case studies—but in the main the introduction matches the terms of reference set by its author. What follows is no less unexceptionable though perhaps somewhat less useful for the student constituency for which this work is intended. Chapters on Japan and China examine the course of critical political change though in the case of the former state the survey ends in 1947. There follows a general inquiry into the position of the Overseas Chinese which poses the problem in terms of absorption, succeeded by two studies of social and political change in Vietnam and Indonesia in which the former chapter by Peter King is the more worthy synthesis. Finally, the relevance of African experience is applied to the political condition and prospects of Australian administered New Guinea. With the possible exception of this last chapter, it is doubtful if students will find anything in this collection which is not available in more original and stimulating form elsewhere. However, providing they are not so complacent as to conclude that all their intellectual needs in this field will be satisfied within this set of hard covers, this volume is not without merit as an introductory text.

Michael Leifer

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Colonialism in Africa 1870–1960. Volume 3: Profiles of Change: African Society and Colonial Rule. Edited by Victor Turner. (Cambridge: Cambridge University Press, 1971. Pp. viii, 455. $17.50.)

This is the third of five volumes to appear in the survey of Colonialism in Africa 1870–1960 under the general editorship of Peter Duignan and L. H. Gann. Unlike its two predecessors, in which historians concentrated on the aims and activities of the colonizing powers, the most recent volume focuses on Black Africa itself and the impact European colonialism made on various aspects of its societies and cultures. What is there for a political scientist in this collection written primarily by social anthropologists for the benefit of historians?

First, there are several important things he will *not* find in the book. He will not find a representative survey of what happened to different sorts of societies under different sorts of rule in different parts of Black Africa. While no single volume can provide ideal coverage, the concentration on English-ruled areas, particularly of East and South Africa, does seem unnecessarily heavy. Only one contributor deals specifically (and well) with French rule—some references by other writers to French practices suggest they are not on their home ground—and the Belgians and Portuguese get only passing mention. Neither will the political scientist find a set of ready-made and agreed upon categories for the analysis of externally imposed social change, nor a general overview of the colonial impact on just Africa. The editor, out of wisdom, discretion, or desperation, reports that he tore up his attempt at synthesizing his contributors' efforts and limited himself to a five-page, and oddly defensive, introduction.

The book's main interest for the political scientist lies in some excellent, primarily political case studies of what happened to particular societies under colonial rule; three synthesizing studies of subjects that have not received enough attention from political scientists dealing with Africa and one superb general essay by Max Gluckman that stands in a class by itself. In the case studies Ronald Cohen traces the effects of British rule on the Bornu Empire of Northern Nigeria; Martin Klein (the lone historian represented) shows how French suzerainty transformed the role of the chief in Senegal's Sine-Saloum area in the years before 1914; John Middleton and Peter Rigby each deal with the effects of colonial regimes' introduction of formal political chieftainships into small-scale East African societies (Lugbara and Ugogo) in which formal authority had previously been decentralized and closely linked to ritual-performing roles. The authors are on very familiar ground, each having previously written extensively about the society in question.

The most useful of the more general, synthesizing studies are those by Elizabeth Colson on how colonialism affected land rights (the best short discussion of this much analyzed problem I have seen), Aidan Southall on imperialism and urban development (Michael Banton's related piece on urbanization and the color line is less successful), and F. B. Welbourn on African responses to missionary stimuli (a strong chapter which badly needs a companion dealing with the responses to Roman Catholic missions, particularly in West and Central Africa).

The reader of these contextually rich studies will draw many tentative generalizations of his own. This reviewer was particularly impressed with the repeated illustration of how European rule rigidified once flexible African social and political systems; for example, imperial rule reduced the ability of both the large-scale Bornu Empire and small-scale Ugogo to deal with human physical movement, particularly of herdsmen—as the Europeans brought in a conception of order utterly dependent on a fixed relationship between geographical boundaries and political authority. The insistence on a symmetrical and hierarchical system of government frequently increased African political authorities' power in some of the least socially useful spheres by removing ritual and other checks and balances and indirectly encouraged them to extort larger amounts of wealth from their subjects, or at least to retain what they received for themselves. As Colson and Southall show, the most flexible and broadly progressive uses of rural and urban land came about in those areas where European influence was least felt. Are such rigidities in any way "necessary" aspects of the development of centralized rule? Are they peculiar to European imperialism, or might any outside intruders have produced such results? Studies of this sort cannot tell us, but they do call attention to the need for historically and culturally comparative work to attempt general answers.

A useful step in such a comparative direction is made by Max Gluckman's "Tribalism, Ruralism and Urbanism in South and Central Africa," a paper whose title belies the writer's intentions to sum up some of the principal political lessons of his many years' experience as an anthropologist in Africa. The essay brings together straightforward theory, evidence filtered through experience, and comparative data from other places and other times. In short, it does what is necessary to arrive at solid general conclusions. The clear statement of his terminology and the application of his familiar conflict and equilibrium model of social relations make it possible for others to pinpoint areas of disagreement on facts or interpretation and assess their relevance to the structure and conclusions of the argument as a whole. The brief comparisons of contemporary African situations with pre-twentieth century European situations add a useful sense of perspective missing in the papers of other authors in the book. (Thus, Southall in particular might have recalled that, however much it helps, imperial conquest is hardly an historical requisite for the construction of appalling urban slums.) Gluckman's political conclusions, pointing to continued relative stability for South Africa and sterile instability for much of Black Africa, are likely to make most of his readers just as unhappy as they quite evidently make their author. The theoretical implications of his analysis, indicating that "we need to abandon the view that in these kinds of colonial societies the principal social factor was the forceful domination of self-interested alien minorities" (p. 162), will make some scholars and many ideologues even more unhappy. Let us hope that in attacking his position his critics show the same high standard of analysis that Gluckman displays.

William J. Foltz

Yale University

Decolonization, the Administration and Future of the Colonies, 1919–1960. By Rudolf von Albertini. (Garden City, N. Y.: Doubleday & Company, Inc., 1971. Pp. 680. $12.95.)

The author of this substantial volume sets forth as the subject of his inquiry a survey and analysis of the attitudes of European countries, and notably Britain and France, toward their colonies during the four decades following World War I. In these decades colonial administration had to be re-oriented, decolonization came to general acceptance, and new relationships had to be worked out between the colonial power and its former colonies. He is explicit in stating that he is not undertaking a contribution to the history of Asian and African states, which he sees as beyond his scope, but is examining an important aspect of European history. In consequence relatively little attention is given either to the actual administratign of colonies and the changes therein over the decades or to the rise of nationalist movements. The material on which he has drawn consists primarily of official British and French pronouncements, of party programs, and of speeches and writings of influential political leaders, colonial administrators, and other commentators. It might be remarked that much of this material overflows into the abundant footnotes which occupy 113 pages.

Von Albertini sees decolonization as having been hastened by many factors, but finds its principal source to be the process of modernization instigated by the colonial powers. "This process in turn was simply a part of current world technological revolution and the spiritual and social changes that accompany it. Colonial conquest itself stimulated the forces which would one day lead to emancipation" (p. 525).

He rightly warns, however, against any assumption that the colonial powers embarked at an early stage on a purposeful and ethical policy of decolonization. Indeed, one of the most interesting, and somewhat startling, features of the book is the repeated citation, from World War I up virtually to the abandonment of colonial rule, of authorities of all varieties who insist that independence is not presently fitting or tolerable. Whether they arrived at this conclusign because of a desire to cling to the fruits of empire or because of the conviction that the "natives" are not competent to run their own affairs must remain among the puzzles of the legacy of colonialism.

As it became increasingly evident that colonialism's days were drawing to an end, an effort was made in a number of quarters to evade or postpone the ultimate iusue. For the British the tendency was to move with all deliberate speed to some in-between stage of self-government or partnership, while the French moved toward assimilation and integration but shied away from the drastic consequences of any full-scale implementation of them. Both countries also in their different fashions invoked the device of a Commonwealth or Union or Community which would have the look of equality for members, granting local control of local affairs, but continuing to vest the major concerns of sovereignty in the imperial center. In both instances the effort notoriously failed as the demand for full independence proved irresistible. The French Community dissolved almost totally as a formal institution although the ex-French colonies in Africa, save Guinea, rallied around de Gaulle and looked to Paris as part of their common heritage. As for Britain, von Albertini sees the solution pragmatically worked out by and for India as the turning point for the transformation of the British Commonwealth of Nations into a multiracial Commonwealth.

A central theme throughout the book is inevitably the familiar difference between the British and the French in their basic colonial philosophy, the French looking to assimilation through direct rule, the British to the development of distinct societies through indirect rule. Von Albertini is, of course, well aware that the distinction is far from absolute and that in the actual working out of colonial administration the two often responded in similar fashion to similar challenges and situations; but this reviewer is persuaded, as the author appears to be, that there remains a fundamental difference in approach. In relation to decolonization, with freedom and equality replacing domination and super-imposition, von Albertini portrays the French as setting their goal in terms of the individual who finds his freedom within the state and society which embraces both the mother country and the former colonies, while the British set out from the separate communities of mankind and find freedom in the attainment of sovereignty and independence. Although the author abstains from openly taking sides, it seems reasonably evident that on the whole he finds the British position more acceptable although he has no illusions as to the shortcomings of indirect rule.

This book is based on a wide variety of sources, covers a large amount of ground, and maintains a high level of scholarly moderation and sensible interpretation. It is very useful to have such a book available because of the quantity and quality of the material it brings together between two covers on a topic of marked current interest, including four brief chapters on the colonial policy of the United States, the Netherlands, Belgium, and Portugal. This is not, however, to say that the book adds very much to the existing stock of knowledge in either interpretation or substantive material.

One further critical comment which must unhappily be made is that the book has been carelessly edited, letting errors slip in which cannot help but raise doubts as to the accuracy of other statements and citations. To indicate errors which caught this reviewer's eye: the terminology of mandates and trusteeships went sadly wrong; for example, the League's Permanent Mandates Commission had to present its reports through the League's (nonexistent) Advisory Council and "could reach government level only through this body" (p. 8); the UN Trusteeship Council, "made up of both mandated and non-mandated territories," sent out "special delegations" whose reports were submitted to the "special commission of the General Assembly"; the Committee on Information from Non-self-governing Territories "was formed to force the colonial powers to give political information and to fix 'timetables' for independence" (p. 24). Kaduna, Nigeria, becomes Kadvan (p. 221); the date of Algerian independence is wrongly given as 1960 (p. 335); "the legendary Van Vollenhoven" becomes even more legendary when he is made Governor-General of Indochina for 37 years (actually 1914–15) p. 338; and Taft reports on the Philippines between 1900 and 1912 to President Theodore Roosevelt (p. 475).

RUPERT EMERSON

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The Chinese Road to Socialism. By E. L. Wheelwright and Bruce McFarlane. (New York and London: Monthly Review Press, 1970. Pp. 256. $7.50.)

This is a well-written book. The goal set by the authors is to assess the impact of the Cultural Revolution on China's economic policies, planning, technology, and production. The book consists of 12 chapters and is divided into three parts. It starts with a general survey of Chinese economic strategy up to 1965, containing facts which are already familiar. The second part traces the origin and the development of the Cultural Revolution and the impact of the Cultural Revolution on planning and financial policy, moral incentives as well as technological policy. In this part, the authors provide some interesting observations based on their separate tours to China. The concluding part summarizes the authors' view on Maoists' strategy of development and what they conceived as the Chinese "road to socialism."

The main argument is that Chinese economic performance cannot be judged by pure economic standards such as real productivity per man-hour or rate of return to the state on invested capital because noneconomic aims are being "fed" into the planning system. As the authors see it, the aim of Maoist socialism is not the maximization of economic growth but the building of a new society in which equitable distribution of income among various segments of the population, particularly the peasants and the working class in the hinterland, is more fundamental. They believe that "a genuine attempt is being made in China to construct a new morality, a new moral basis for building a socialist society, and on a scale unprecedented in the whole of human history" (p. 214).

Evolving from such a theme, the book is primarily devoted to the economic justification of every major act of the Maoists from the Great Leap and the commune to the Cultural Revolution. The Great Leap which has been condemned by most experts on the Chinese economy was hailed by the authors as a kind of "crash industrialization" program in the context of developing agrarian socialism without large-scale labor transfers to the cities. The development of small industries is viewed by the authors as a great success. They have quoted official statements issued during the heyday of the Great Leap to support this view and ignored the self-criticism made after the tragic collapse of the Great Leap.

The commune is regarded by the authors as "a means of improving the division of labor in the agricultural sector" (p. 49) with "the advantages of large-scale cooperation for irrigation and canal building and for water conservation" (p. 49). To support their view, the authors believe there was a 30 per cent increase in food production in 1958, which implies 240 million-ton grain output in 1958. No one in the field of Chinese studies could possibly accept such a figure. In 1970, after nine consecutive bumper harvests, the official figure for foodgrain production was only 240 million tons (based on Edgar Snow's interview with Chou En-lai, *The New York Times,* March 13, 1971), while the Chinese population went up more than 100 million during the same period.

As to the Cultural Revolution, the authors reject the concept of power struggle, or a clash of personalities. They see it as "revolt against the power system which industrialization engenders" (p. 28). The aim of the Cultural Revolution, they say, is to promote the human factor in economic development by breaking down the great gulf between the senior elite group of technocrats, managers, and party administrators on the one hand, and the mass of peasants and workers on the other (p. 15). Once again, the authors ignore the fact that "ideologists" such as Chen Po-ta who led the Cultural Revolution up until 1969 now share the same fate as Liu Shao-chi, whom the authors brand as a leading "rightist" in economic policy (p. 88).

There are a few minor errors in the book. On page 129, the Cultural Revolution Group of the Central Committee is said to have been "headed by Mao, Chiang Ching, Chou En-lai, Chen Ta [should be Chen Po-ta] and Kang Shen[g]." This is not correct. Mao as the chairman of the CCP has never headed the Cultural Revolution Group. Nor has Chou En-lai been a member of the Group. The Group was led by Chen Po-ta with Chiang Ching as his deputy and Kang Sheng as adviser. On page 32, the authors state that output per man and per acre in Chinese agriculture was very low. This is only partially true. Although output per man was low because of the extremely small per capita holding of land, yields per acre were by no means lower in China than in advanced countries. In the early 1930s, for instance, yields per acre of rice and wheat were higher in China than in the United States and the USSR. On page 170, the 12,000-ton hydraulic free-forging press is said to have been produced by the Shanghai Heavy Machine Building plant; it was, in fact, made by the Kiangnan Shipyard, one of the oldest firms in China.

Despite this, the book is well organized, adequately documented, and very informative. It offers useful summaries of the developmental strategies during the past two decades and of the Cultural Revolution. It also provides much firsthand information on the micro level—information derived from the authors' brief tours to China in 1966 and 1968. Fragmentary as they are, the data give some insight into the operation of the commune system and the struggle between the revisionist and the revolutionary lines in the factory level before and during the Cultural Revolution. A valuable appendix consists of a collection of ninety-nine "latest instructions" issued by Mao Tse-tung between August 1966 and July 1969. The instructions not only may serve as a gauge to Mao's influence on internal development but also illustrate his basic ideas in reshaping the Chinese society.

CHU-YUAN CHENG

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The OAS in Transition. By M. Margaret Ball. (Durham, N.C.: Duke University Press, 1969. Pp. 721. $19.50.)

The appearance of this important, monumental, and authoritative work on the oldest regional international organization—The Organization of American States—is much to be welcomed, particularly from the pen of such a noted authority as Professor M. Margaret Ball. In her preface Professor Ball declares that the book concerns "the organization and function—and problems of the OAS yesterday, today, and tomorrow," and candidly admits that writing about an organization in the process of undergoing a major transition "has not been a simple one" an admission any person even remotely acquainted with the complexities of the OAS will readily understand.

The author has divided her book into three major sections: (1) Evolution, Principles, and General Structure of the OAS; (2) Organs and Agencies; (3) Activities. Her arrangement of material might raise some question until one has read the book and appreciated the skill with which the separately constructed parts have been linked together.

The first chapter traces the growth of inter-American organization beginning with Bolivar's dream and ending with the signing of the 1967 protocol which drastically revised the Charter of the OAS. In her discussion of the principles underlying the inter-American system, Professor Ball points out that while such principles as equality of states, the rule of law, pacific settlement of disputes, nonrecognition of territorial conquest, nonintervention, self-determination, the promotion of representative democracy, respect for human rights, collective security, and general responsibility for the raising of living standards do not differ materially from those of the United Nations or other regional organizations, nonetheless they carry more import in the inter-American system. "[T]he statesmen of so many OAS members have a philosophical bent which makes them prone to operate in terms of principle rather than on a pragmatic basis . . . [and] to view the OAS in juridical, rather than political, terms" (p. 44). The organs and agencies which implement the principles and purposes of the OAS are carefully explored, including those agencies which, although considered basic to the international life to the Western Hemisphere, have for historical or political reasons remained outside the principal structure of the OAS.

Part II begins with a study of the regular Inter-American Conferences—the former basic policy body of the OAS—and discusses the shift of this organ's functions to the newly established General Assembly. In reviewing the specialized conferences, the author points out that with the ratification of the Protocol of Amendment, the way will be cleared for substantial improvement in the coordination of the activities of the specialized conferences to prevent overlapping and duplication of effort. The Meetings of Consultation of Ministers of Foreign Affairs of the American Republics, which in fact evolved into the principal instrumentality for OAS policy determination, are evaluated by highlighting the important results of the major conferences. Professor Ball concludes that these meetings are likely to remain an important element in the inter-American picture even though they will undergo some adjustment in view of the creation of the new General Assembly.

As the heart of the new amending protocol of 1967 relates to the changes made in the composition, functions, and structure of the old OAS Council and the Pan American Union, the author makes a detailed comparison between the old council and the new Permanent Council, the old Pan American Union and the new General Secretariat, reviewing the composition, the general and special functions, the organizational set up, and the procedures of each. The new status of the technical councils, that is the Inter-American Economic and Social Council, the Council of Jurists, and the Cultural Council (now upgraded to the Council for Education, Science and Culture) is meticulously surveyed. Complete coverage of the evolution, general structure, programs and activities of the specialized organizations dealing with health, women, children, geography, history, Indians, agriculture, and statistics is also included. Nor does the author overlook the other important inter-American agencies such as the Inter-American Peace Commission, the Commissiion on Human Rights, the Defense Board, and the Development Bank.

In the final portion of the book, Professor Ball, in a remarkably complete fashion, investigates the major activities undertaken by the OAS, including the settlement of disputes, the military and political defense of the hemisphere, the support of representative democracy, the protection of human rights, the efforts to extend suffrage, and inter-American economic and social cooperation. The sole criticism one could level at this portion of the book is the scant attention given to the extremely important work of the Inter-American Human Rights Commission during the Dominican Republic Crisis of 1965. But this tale was undoubtedly left untold not because of oversight but because in a volume of this size a halt must be called somewhere.

This scholarly study, with its masses of complex detail, is enhanced by the author's style and clarity. An excellent table of contents, a skillful index, an appendix containing the important documents of the OAS, and a twenty-nine page bibliography add to the usefulness of the book as a reference work. But its appeal is wider since it contains so much of special value to those directly concerned with or intensely interested in the international functioning of a regional system. In short, this is a good book for reading, a rewarding book for study, and an unduplicated source book for reference.

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The Foreign Affairs Fudge Factory. By John Franklin Campbell. (New York: Basic Books, 1971. Pp. ix, 292. $6.95.)

Few investigators of government foreign affairs organization have raised fundamental issues in as stimulating and provocative a way as John Campbell. Thus his sudden death at age 31 is a particular tragedy. His book gives new life to old but important questions, and we shall miss his clear voice in the debate over their resolution.

Campbell was a career Foreign Service officer who took a leave of absence to study the "system" in which he had served. The critique he produced has two targets. The first is organizational—"excessive size and fragmented authority" (p. 229) in government foreign policy-making institutions which have confused operations and weakened State Department influence. What we have, he argues quoting Joseph Kraft, is not a decision-making instrument but a "fudge factory." The second target is substantive—an interventionist foreign policy which has exaggerated both our interests and our ability to remake the world in our image. Campbell felt that the organizational flaws were partly responsible for the substantive, and that his proposed reforms would help ameliorate both. He would strengthen State by drastically reducing its size, and by internal reforms aimed at "more clearly fixing responsibility in departmental business" (p. 233). He would give the Department "new authority over the forty-odd other agencies which play partial roles in foreign affairs" (p. 41). And he would sharply curtail overseas military, propaganda, aid, and intelligence activities. With the foreign affairs government thus dominated by men possessing a realistic (rather classical) appreciation of the nature of interstate relations, the U.S. could avoid recurrence of recent disasters.

As Campbell himself notes, his basic argument is "not entirely new" (p. 41). In championing the "old diplomacy," he cites Harold Nicolson, George Kennan, and Jules Cambon. His organizational prescription draws heavily on the report of the 1949 Hoover Commission foreign affairs task force. Thus critical reviews in the reform-minded *Foreign Service Journal* have characterized the book as "The Remembrance of Things Past" and "Metternich Revisited," and the major defense has come from retired Ambassador Ellis Briggs. Much of Campbell's critique of the "new diplomacy" seems warranted, especially in the light of Vietnam. Yet his organizational prescriptions tend to be based more on what seems intuitively reasonable than on any careful analysis of how they would affect the actual patterns of influence within government.

It is clearly plausible to argue, for example, that since many of the problems of managing the foreign affairs bureaucracy are related to the large number of officials who must be "coordinated," to have fewer officials should make things better. But assuming staffs will remain quite large, how much effect on the nature of the policy-making system will specific reductions have? Why should we expect that Campbell's 3,000-man State Department won't have many of the same problems of "elephantiasis" that the 7,000-man Department has today? And how will the personnel reduction process Campbell advocates avoid the same pitfalls he cites in recent Johnson and Nixon Administration efforts—placing seniority over ability, or featuring interagency bargaining on an "all programs are equal" basis rather than imposition of a rational set of personnel priorities? Thus one must ask a further question—whether such sweeping condemnations of size *à la* Galbraith are not a retreat from serious analysis of the problems of managing complex—and almost by definition large—modern organizations.

Campbell would doubtless respond that he did not limit his proposals to reducing numbers. He also stresses organizational simplification. He calls for eliminating nine of State's present sixteen bureaus, leaving only the five geographic regions, administration, and a proposed "Bureau of Multilateral Affairs." Yet while this would have the virtue of reducing lateral clearances, it would tend to make the Secretary of State the captive of regional, "country desk" and ambassadorial viewpoints, even more than he is today. This would scarcely enhance his acceptability as government-wide foreign affairs leader, either to the President or to his Cabinet colleagues. Yet the book does not face this sort of question.

Finally, one must question the way Campbell would reverse "the preponderance of military influence in foreign policy" (p. 197). He seems to consider "foreign policy" to be a relatively narrow and clearly delimited preserve from which military poachers must be excluded. Thus, just as the Hoover task force criticized the National Security Council for getting "into numerous matters of foreign affairs which are strictly not its business," Campbell urges abolition of the Defense Department's foreign policy staff—the Office of International Security Affairs (ISA). Yet one has only to go down the list of major U.S. foreign policy issues today—Vietnam, China, Europe, Japan, arms control talks with Russia—to see that virtually all of them have strong military components. If the State Department really wants foreign affairs primacy, it must seek not to keep the military out of its own "diplomatic" business, but rather to get its own people more effectively into the military business. To do this, State must strengthen (not abolish) its own political-military affairs staff, and must build alliances with offices elsewhere (like ISA) which share its interest in subordinating military strategy to foreign policy.

Campbell is right in arguing that there are strong reasons for shifting the foreign policy leadership role from the N.S.C. staff to the Office of the Secretary of State. He is wrong to suggest that a State Department responsive mainly to its own traditional values and priorities could attain either the substantive expertise or the bureaucratic clout to bring this about. His book is nonetheless valuable for the issues it raises, and for the clarity and persistence with which its major proposals are advanced.

I. M. Destler

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De Lege Pactorum. Essays in Honor of Robert Renbert Wilson. Edited by David R. Deener. (Durham, N.C.: Duke University Press, 1970. Pp. xiii, 274. $13.50.)

Against all odds in this type of academic ritual (essays in honor of somebody), this is a well-focused and well-edited collection of 16 essays on the law of treaties, all especially written for this volume.

The law of treaties has probably been the most "in" topic in international law in the last few years. The 1966 Draft Articles of the International Law Commission and the 1968 and 1969 Vienna Conferences on the Law of Treaties, eventuating in the 1969 Vienna Convention, have prompted a quantum leap in scholarly concern for treaties. In October 1967, two of the oldest established journals in the field, the *American Journal of International Law* and the *Zeitschrift für ausländisches öffentliches Recht und Völkerrecht*, saw fit to depart from their traditional format to put together a special issue on the law of treaties. Two worldwide bibliographies on treaties have recently been prepared: one by the United Nations Secretariat (A/CONF. 39/4, 1 Feb. 1968, 150 pp). and the other by the Peace Palace Library at The Hague Academy of International Law (mimeo, 1968, 94 pp.). And the National Science Foundation has helped in bringing at least one part of this information explosion under control by funding the Treaty Information Project at the University of Washington.

Even viewed amidst this scholarly bumper crop, the present collection makes a distinct contribution. Its team of authors has been carefully chosen. Robert Wilson, in whose honor the volume has been published, is one of the world's leading authorities in the field of treaties and agreements. David Deener, the editor, is a widely known specialist in the relation between international agreements and domestic legal processes. All contributors, including the editor, have at one time or another studied under Robert Wilson, and have since continued and developed their teacher's interest in treaties. Deener's excellent introduction presents

Wilson's career in a sketch of how international lawyers have responded to the world around them during the last half century. Since space does not allow to review all sixteen essays I have selected the following five on the grounds that they are likely to appeal to a wider range of political scientists than just the international law fraternity.

(1) Hans W. Baade, " 'Constitution-making Treaties' and International Courts: Judicial Review Within the European Communities," pp. 19–39. A few specific examples show the ever-increasing complexity of the subtle interlacing of unitary, federal and international legal processes in the European Communities. The whole essay represents a giant step in sophistication beyond the centralization vs. decentralization model usually applied to the emerging European system.

(2) David R. Deener, "Treaty Norms and Statutory Norms: Patterns of Priority and Choice," pp. 73–92. This is a masterly synthesis of the interplay of domestic and international legal rules. An imaginative matrix shows all possible dyads for and against priority rule among plaintiff, defendant, court, treaty, and statute. The treatment is richly comparative. It covers constitutional provisions and scholarly writings for many more countries than are usually surveyed in writings on Anglo-Saxon jurisprudence.

(3) Conley H. Dillon, "The Role of International Labor Conventions in Economic Development in Emerging Nations," pp. 93–111. Drawing on many years of expertise on related matters, including a book on ILO conventions as early as 1942, the author now reviews the impact of ILO conventions on domestic labor standards and legislation in African, Middle Eastern, and Asian countries. He develops an intriguing "conformity score" according to which Africa leads the group scores with 90 per cent followed by the Middle East with 75 per cent and the Far East with 61 per cent. For comparison, Western Europe shows 71 per cent and Latin America 44 per cent.

(4) R. R. Oglesby, "Succession to Treaty Responsibilities in Civil Wars," pp. 185–201. This is a timely essay with clear relevance to legal-political writings on Vietnam, but it does not mention Vietnam as such. The range of examples stretches from the American War of Independence to that of Algeria. It shows that insurgents of various times and places have compromised somewhere between the "clean slate" and the "full continuity" doctrines, as is generally true for the broad interface between new states and international law and politics.

(5) Claude S. Phillips, Jr., "The Bilateral Treaty Network of Non-Western States," pp. 202–232. This is the only piece in the group that is primarily quantitative. I should declare my general bias in favor of such new methods in international legal studies and my part in supplying the author with statistical data from the Treaty Information Project. Having said this, I feel free to add that this paper would strike even a hard-core traditionalist as an imaginative piece of work with thought-provoking tables showing the comparative infrequency and the peculiar patterns of treaties among non-Western states.

The other eleven essays are by Alona Evans on self-executing treaties; Mohammed Chaudri on reservations to multilateral treaties; Robert Clute on treaties on outer space; Charles Hagan on weighted voting in commodity agreements; John Howell on reservations in dispute settlement agreements; Ralph Jones on the consensual basis of international law; Alan Leonhard on teleology in treaty interpretation; Gary Maris on leasehold agreements; Don Piper on special agreements for atomic ships; William Simpson on treaties, tariffs and cotton; and Herman Walker on U.S. commercial treaties.

All in all, this collection will be particularly useful to treaty specialists, but it will also provide new facts and thoughts for generalists in the international field. A graduate seminar or law school seminar might well be experimentally structured around this book.

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Economic Warfare in the Communist Bloc: A Study of Soviet Economic Pressure Against Yugoslavia, Albania, and Communist China. By Robert Owen Freedman. (New York: Praeger Publishers, 1970. Pp. 210. $14.00.)

Economic sanctions as a weapon in international relations seldom achieve the results intended by those who invoke them and often in fact tend to reinforce or create the very situation they were invoked to change or avoid. That this commonplace proposition has also proved valid in the case of Soviet economic pressures directed toward other Communist-ruled states and that successive Soviet leaderships have learned some but not all of the appropriate lessons is the thesis of Mr. Freedman's series of case studies of economic warfare in the Balkans and the Far East since 1948.

Yugoslavia, Albania, China, and North Korea (the last is briefly considered in the chapter on China) were all in varying degrees "vulnerable" to Soviet economic pressures when Stalin or his successors applied them in efforts to maintain Soviet hegemony—or later merely unchallenged Soviet supremacy—within what Mr. Freedman alliteratively calls "the Communist camp." Each of these countries was hurt by Soviet sanctions, with Yugoslavia in particular being brought close to economic collapse, but each found alternative sources of trade or aid which brought the price of resistance down to levels that determined regimes and peoples who were responsive to appeals to national patriotism were willing and able to bear.

Besides the general observation that such sanctions are never likely to succeed if the victim has or can find access to alternative markets and creditors and that they can prove dangerously counterproductive if the alternative supplier is a competing power eager and able to capitalize on its opponent's mistakes, Mr. Freedman draws our attention to several more specific conclusions to be drawn from the tale he tells.

Perhaps the most interesting of these, if only because its obviousness has been curiously overlooked by many writers in the field, is that one reason for the failure of Soviet experiments with intrabloc economic warfare was the gradualism with which Stalin escalated his economic sanctions against Yugoslavia over a period of eighteen months in 1948–49, a strategy repeated by Khrushchev when it was the turn of Albania and China a decade later. In each case, Mr. Freedman argues, this gradualism is explainable in terms of Soviet overestimation of the impact of the mere threat of sanctions and underestimation of the unity of the Communist leadership of the "target" country and the willingness of both leaders and peoples to give higher priority to national independence than to a high economic growth rate. In the Albanian and Chinese cases, he further suggests, an additional factor was a lesson learned from the Yugoslav experience, when a total blockade forced the Yugoslavs not into submission but into a reorientation of their foreign economic relations that led to enduring links with the capitalist enemy, permanently liquidating serious vulnerability to later Soviet economic pressures. With Albania and China, therefore, the embargo was never complete (credits and technicians were withdrawn, but COMECON states other than the Soviet Union continued to trade with Albania and all of the bloc continued trading at a reduced level with China), presumably to avoid forcing them into the "enemy camp" and to make the path to eventual reconciliation easier. The difference may also have reflected the changed and more modest goals of Soviet intrabloc policy under Khrushchev, who recognized the limitations of Soviet power over states with independent Communist revolutions and who sought supremacy in place of the absolute hegemony that had eluded Stalin in this wider Communist Commonwealth.

Mr. Freedman has produced a competent and useful addition to the growing shelf of specialized case studies, dealing with specific aspects of Communist policies and societies, in which material of relevance to the subject is conscientiously collected from existing secondary sources, collated with appropriate selections from standard translation services like Radio Free Europe and *The Current Digest of the Soviet Press,* and added to an original perusal of one or more specifically relevant periodicals. In this instance the principal primary source is *Vneshniaia Torgovlia,* the organ of the Soviet Ministry of Foreign Trade.

While such studies may tell the Communist area specialist little he did not already know, they assemble, focus, and analyze a significant segment of that information in a convenient and useful form. Among nonspecialists Mr. Freedman's study will also be of interest to students or practitioners of the games people play in contemporary international politics. Such people may join Mr. Freedman in wondering "whether a high official in the Soviet Union's Ministry of Foreign Affairs, in examining the USSR's use of economic pressure against fellow communist states over the past twenty years, would come to the same conclusion as to the efficacy of economic pressure as a foreign policy instrument as did Dean Acheson," whose comment on similar American experiences he quotes: " 'Almost all recipients of our economic or military aid have shown us how useless threats to stop aid are in trying to pressure the recipients' " (p. xv).

The book is written in a fashionably workmanlike if pedestrian style marred by occasional colloquialisms or neologisms (e.g., "rehash" on p. xv, "factionalize" on p. 151) and inconsistent use of throwbacks to the now unfashionable phrasing and terminology of the partisan scholarship of the Cold War epoch (as in "Communist camp" and a chronic uncertainty as to whether the Council for Mutual Economic Assistance should be called COMECON or CMEA). There is also some unnecessary repetition not only of arguments but also of quotations, some of them lengthy (on p. 5 repeated on p. 39, pp. 8/131, pp. 10/125f, pp. 11/27, *et al.*).

Finally, while the temptation to deduce what a leader like Khrushchev or Tito personally thought or intended from the policies his Party or government pursued is sometimes hard to resist, especially in writing about a relatively closed society where individual roles are often obscured, to state categorically (for example) that in 1962 "Khrushchev's interest in the renewed rapprochement [with Yugoslavia] was primarily political... [but] Tito's reasons were more economic in nature" (p. 46) requires more justification than is offered. The same is true of the contention that "as an added form of pressure on China, Khrushchev embarked on a policy of rapidly increasing both trade with and aid to India—including substantial amounts of military assistance—in an effort to help develop India as an alternate power in Asia" (p. 149; cf. p. 139: "the Soviet Union under Khrushchev was trying to build up Indian military power to help balance the Chinese in Asia"). Tito has usually given political (or even ideological) considerations a higher priority than economic, and at the level of weakly substantiated speculation one could equally well argue that the primary motivation for Soviet aid to India was a desire to break through the SEATO link in Western "imperialism's" encircling containment or to build Soviet influence in the Third World on Indian foundations.

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The Future of the Oceans. By Wolfgang Friedman. (New York: George Braziller, 1971. Pp. 132. $5.95.)

Professor Friedman writes that his book "... is not a neutral book." His somewhat alarmist attitude can be seen in his conclusion, on page ii of Preface, "To remain silent or passive toward such a development [partition of sea-bed and oceans] is to participate in the destruction of the principal achievements of international law in the last few centuries and to accept the preparation of an Orwellian nightmare." It is difficult for this reviewer to reach such a dramatic conclusion on the basis of the material in the book. The author disregards the fact that since its codification in the beginning of the seventeenth century, many of the original precepts of international law have been modified, and where the law proved obsolete, either completely discarded or rewritten. No basic law has been enacted on the exploitation of the sea. There was no Grotian admonition, "*cujus est solum ejus est usque coelum,*" (he who controls the land controls the air above it up to the heaven) applicable to the control of the depth of the seas, nor was there any mention made of continental shelf, slope, trench, etc. Whereas the laws governing the surface navigation of the seas developed over many centuries, exploitation of the ocean-bed is in its frontier stage.

The book, which has an index and bibliography, is divided into ten very short chapters, apparently intended to present limited summarizations of existing literature rather than to offer additional data. Professor Friedman restates arguments presented by many other publicists that the resources of the seas are *res communis* (communal property) not *res nullius* (belonging to no one), and therefore, are not within the domain of a small number of coastal states. This assumption is based on the fact that these mineral and metal deposits flowed into the seas in form of sediment carried by rivers which drained the coastal territories.

The author all too abruptly dismisses all the earlier efforts on the exploitation of ocean-bed resources. His attention is focused on the new era which began in 1945, with the Truman Proclamation of the Continental Shelf. He justifiably stresses the technological progress made since 1945 and the increased effort by several nations to exploit ever larger areas of sea floor. This trend has accelerated the danger of military and political struggle for the control of these resources. To prevent such tragedy an international agency, independent of the United Nations Organization, must be established to administer and enforce a universally acceptable set of regulations. Friedman estimates the potential deposits of minerals, metals, and food that seas could provide. He supports the program advocated by a British agricultural expert, Sir John Boyd-Orr, for the "establishment of a world food authority, that would redistribute vital foodstuffs from surplus to deficient areas" (p. 13). It is very difficult to visualize such a distribution of food produced at high cost from the seas, since food produced on land at a much lower cost is now withheld from starving nations either because they lack purchasing power or because they refuse to accept the political, military or ideological viewpoint of the donor.

The author clearly reviews present exploitation of the ocean-bed, pointing out that one-sixth of the nations could profit directly from underwater resources but only six nations can afford such luxury. Under present international law one-fourth of the nations have no claim to any of these resources, because of their geographic location. The author also indicates that ecological pollution which would result from sea mining would destroy much of the undersea food supply unless new methods were discovered and employed. This inevitably would result in higher production costs and would limit the number of participating states. Present technology for the recovery of metals and minerals from the ocean-bed at depths greater than 300 meters is too costly to be practical. There is no indication that technological progress in the foreseeable future will make sea mining at great depths an economical venture.

The author believes that before any significant effort can be made to share these resources among all nations, contemporary views on territorial seas, on freedom of the seas, and on the rights of land bound states must be changed. Probably even more important, military uses of the ocean-bed must be resolved first. The Russo-American convention on the prevention of installation of nuclear or other weapons of mass destruction on the ocean-bed is an attempt in the right direction, he feels. Similarly, the establishment by the United Nations General Assembly of a forty-two nation commission to study these problems in 1968, and the United Nations Resolution of 1970, which doubled the membership of the commission and received majority support in the United Nations, are welcome signs of united activity to resolve these issues. Professor Friedman, however, does not attempt to analyze the plans presented by the United States, Soviet Union, Malta, and Tanzania.

Construction of the Mont Blanc tunnel, the Moselle canal, and organization of Air Afrique are cited by the author as examples of international cooperation to solve common problems. It should be remembered that the latter projects are insignificant in scope, legal technicalities, technology, and capital investment compared to the problems entailed in the exploitation of the ocean-bed by the international community.

Despite its many limitations, this book is a contribution to the study of the future of the ocean-bed exploitation in that it focuses the attention of the reader on the pressing need for immediate effort to resolve the issue at hand, and that it offers some suggestions for an equitable solution of the problem by the United Nations.

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The United Nations in International Politics. Edited by Leon Gordenker. (Princeton University Press, 1971. Pp. 241. $7.50.)

Although the United Nations is accepted as a force in international politics, what standards are to be used in determining the degree of its influence and its value, and what research is needed to aid our understanding of the past and to forecast its future? Around these questions the six essays of the book offer a theoretical framework for seeking answers that focus on the central problems of the United Nations operating to establish international peace and security. The contributions of each of the authors reflect their association with the Center of International Studies at Princeton University. Their points of view are related in a systemic approach which considers the United Nations as a subsystem within the larger system of the world political environment. Each of the individual contributions suggest areas of investigation for the student, and the book as a whole should stimulate more disciplined scholarship among those seeking an understanding of United Nations activities.

The Oran R. Young chapter on "The United Nations and the International System" offers a theoretic framework for exploring various roles and functions of the United Nations Organization. The Organization is presented as a regulator in providing a minimal level of stability; as an effector of great-power agreements; as a partisan political instrument; as a force for political change; as a creator of long-term viability; and, as a creator of norms. In pursuing this analysis Young notes the shift in membership patterns and the significance of these shifts in explaining the underlying axes of political contention in the international system. These axis patterns and their variations, he concludes, indicate a "contextual fluidity" of the international system which offers both pitfalls and opportunities to the United Nations Organization in the roles it is called upon to play.

S. J. Michalak, Jr. provides a comparative outline of the relationships between the behavior of the universal international organizations in the realm of peace and security, and the systemic conditions within which they operate. His comparisons of the behavior of the members of the League of Nations and of the United Nations offers hypotheses for analysis in which the variables indicating homogeneous or heterogeneous policy development operate in conditions of bipolar, multipolar and bi-multipolar systems. His theory seeks to determine which variables are most crucial in exploring the context and differentiations among member states acting in the world system.

One difficulty in the study of United Nations activities is indicated in the G. R. Lande chapter, "An Inquiry into the Successes and Failures of the United Nations General Assembly." That difficulty lies in using the printed or recorded statements of position, or votes of states, in committee and plenary meetings as a basis for revealing the complex interrelationships among states. Is agreement or noncompliance with U.N. recommendations the result of discussion within the U.N., or do factors outside the U.N. in the international system determine the attitude and actions of states? The Lande study appraises 29 resolutions of the General Assembly to determine which "proved successful or partially successful with respect to both implementation and effect and which were abjured and had little or no consequence" (p. 108–9). The framework for analysis is excellent in providing for consideration of methods, procedures, and the behavior of actors in the United Nations context. But, as pointed out in the conclusion, the most telling factor in revealing the efficacy of the General Assembly is the extent of the efforts made by states to explore a variety of courses of action that might best ease the tensions between opposing parties and enlist cooperation from all concerned. Such an analysis requires the complete United Nations record. The written record may be too summary, and the full tape recordings are available only in the original language. The more difficult problem, however, lies in achieving an awareness and appreciation of underlying related issues, often in situations involving other subsystems of the international system. Nonetheless, both the Lande study and the essay by L. B. Miller, "International Organization and Internal Conflicts—Some Emerging Patterns of Response," provide the sort of framework that warrants extensive use in evaluating United Nations activities.

Leon Gordenker emphasizes the political importance of the administrative processes through which international organizations contribute to the adjustment and pacific settlement of conflicting interests. His chapter, "The United Nations and Economic and Social Change," describes the provision of institutionalized means available through the expanding activities of the family of United Nations organs. It presents a general appraisal of the influence that international organizations can exert. Here also, the book clearly shows the need for more such studies to document the viewpoints offered. Fortunately this need is beginning to attract the attention it deserves. The recorded data of United Nations mission, surveys, Secretariat reports, and development consultants' project reports, along with the personal experiences of international and national civil servants in debriefing sessions, can add the dimension needed to indicate the relationships between the peace-building activities in economic and social fields and the peace-keeping responsibilites of the Organization.

Richard A. Falk in the concluding chapter offers "a very preliminary effort to evolve a disciplined procedure for assessing the degree to which the United Nations has met, is meeting, and will meet the needs of international society" (p. 188). His view of the Organization combines descriptive analysis with normative appraisal. He outlines two alternative systems to be tested in identifying the autonomous and semi-autonomous roles of the United Nations. The analysis reflects the gap that separates the United Nations Charter goals and the limited capabilities of the actual United Nations system operating in the international environment. His models in a Charter System and a United Nations System offer a basis for a realistic assessment of the roles of the Organization, recognizing that the assessment must reflect patterns of political influence and must be correlated with implementing capabilities.

"The United Nations in International Politics" offers a real challenge in emphasizing the need to examine critically and perceptively the mass of information generated by the United Nations and to search other relevant sources. The challenge is defined in presenting a comprehensive systems analysis as a framework in which to bring to bear all the talents and skills of a multidisciplinary approach to international politics. Its suggestions for further research should enable us to develop the standards for judging the roles and actions of the United Nations and provide forecasts of future roles for the Organization.

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The Stable Crisis: Two Decades of German Foreign Policy. By Wolfram F. Hanrieder. (New York, Evanston, and London: Harper & Row, 1971. Pp. 221. $3.50.)

In the 'fifties the expression "stable instability" was used to characterize the domestic policy of the French IVth Republic (e.g., by André Siegfried in 1956). Wolfram F. Hanrieder uses the slightly modified term to characterize the foreign policy of the Federal Republic of Germany, and he has indeed coined an impressive and illuminiating formula. In his earlier book, *West German Foreign Policy, 1949–1963: International Pressures and Domestic Responses,* Hanrieder had disclosed his inclination and his talent for conceptualizing the foreign policy of the Federal Republic. His new book covers a longer period and is more of a narrative, but it is clearly different from Waldemar Besson's book (*Die Außenpolitik der Bundesrepublik: Erfahrungen und Maßstaebe* [Muenchen: R. Piper Verlag], 1970), which appeared almost simultaneously.

In three chapters Hanrieder outlines the evolution of the three most important goals of the Federal Republic's foreign policy: security, political and economic recovery, and reunification. In pursuing the first two goals Bonn's expectations were not always fulfilled (as in the Multilateral Force Project), and serious controversies even within the government party could not be avoided (for example, those between the "Atlantic" and "Gaullistic" wings). Nevertheless, the story of the efforts to attain these goals is on the whole a true success story; the third chapter, however, dealing with the "supreme" goal only reports failures. Yet success and failure are most closely connected, for (as Hanrieder suggests) the security pursued was a "Western" security, the rapid economic recovery was a "Western" recovery; these achievements thus *necessarily* excluded a successful solution of the reunification problem, a solution which had to be an "Eastern-Western" success. A fourth chapter, "Foreign Policy and the Domestic Political System," concentrates on the connection between the dilemma in foreign policy and the domestic political system. The question could be asked: Which domestic forces established priorities in foreign policy with a view to which model domestic system—that is, which forces preferred security and recovery within the framework of Western policy to reunification within the framework of an understanding between East and West? Hanrieder does not present the possible answer that the German bourgeoisie placed security first in order to reconstitute the capitalist system, because this answer evidently does not correspond with historic reality. Despite some criticism of Adenauer's policy as being too concerned with small European-Catholic countries, nearly all critics (except the small Communist party) agreed with the Chancellor's maxim that reunification should not be achieved at the expense of security and political-economic recovery. None of the important groups placed reunification "above everything," and the Social Democratic Party, too, rejected a neutrality which would have surrendered a reunified Germany to the pressures and interventions of the neighboring superpower.

In other words, the Federal Republic's decision in favor of the West was not a party decision and cannot as such be reduced to particular social interests. All controversies concerned methods and, at most, degrees, and for this reason the dilemma in foreign policy was a dilemma of West German society as such, and not only the dilemma of a single class or party in this society. Therefore, in this chapter, Hanrieder is on precarious grounds, for he neglects the fundamental consensus in favor of the differences of opinion, so that the Social Democrats appear less "Western" and the right wing of Adenauer's government coalition, especially the small "Deutsche Partei," more nationalist than they really were.

With regard to the present, Hanrieder describes an antagonism which is in no way basic or even ideological, and which cannot clearly be associated with political or social groups: the opposition of "traditionalists" and "reformers" (p. 182). The "traditionalists," according to Hanrieder, stick to the old demands for reunification and its implications, e.g., the Hallstein Doctrine, while the "reformers" want to conduct a "flexible and dynamic" foreign policy on the basis of the status quo now firmly established by international political developments. The shift toward reform of foreign policy was initiated by the Christian Democratic Union Foreign Minister Gerhard Schroeder, continued by the government of the Grand Coalition, and it is now being completed by the Brandt-Scheel government. Hanrieder leaves no doubt that he considers the "New Eastern Policy" necessary and right.

I doubt, however, whether Hanrieder's categories suffice for an understanding of the complexity of possibilities and tendencies which, especially now, are immanent in German foreign policy. It might perhaps be better to distinguish between "accepters" and "nonaccepters." Some of the accepters obviously accept the status quo only because they want to change it with the help of new methods in order to reach the old goal of reunification under Western auspices. Others have put up with the status quo because they consider it essentially unchangeable. A third group may accept the status quo because it is regarded as an intermediary stage on the way to a Communist reunified Germany. On the other hand, some of the "nonaccepters" subscribe to Adenauer's view that by taking advantage of the changing world political situation, and with the backing of a united Western world, it might one day be possible to force the Soviet Union to surrender her German satellite state; for others, revisionism is only the Federal Republic's most advanced line of defense; and a third group feels bound to the principles of international law and democratic ideology. Which tendency or which combination of tendencies will finally gain the upper hand cannot yet be foreseen; at any rate, contrasting "traditionalists" to "reformers" does not do justice to a complex reality. The most interesting and most surprising tendency is doubtless the Communist one, because, although for some time hidden behind demands for academic reform, it succeeded spontaneously in the most active sector of student youth. Hanrieder cannot be reproached for not having noticed in 1969 in the United States what in 1971 does not really seem credible even to the general public of the Federal Republic. But one day it will not be possible to overlook the fact that the genesis of a strong "Party of the East" among the student youth in the model affluent nation of Western Europe was the most surprising event in the history of the Federal Republic, and that in the long run this development might have extraordinary significance especially with regard to foreign policy.

To summarize: Hanrieder's book is readable, concise, and stimulating; the analysis and narrative are well balanced; and furthermore, it contains a very useful bibliography of about two hundred titles. It leads the reader to the threshold of the immediate present of German foreign policy. The book appeared too early to take into account (except for a casual remark on p. 195) that "re-ideologization" which has not yet reached the level of big politics. There exists now, however, a definite possibility that by the acceptance of the status quo, the "stable crisis" of this foreign policy will not turn into a stability free from crises but into an unstable crisis wavering between fundamentally different possibilities of orientation. In his final paragraph Hanrieder seems to express a similar doubt when he asks whether, in the long run, pride in economic achievements can be a substitute for a "genuine national sense of purpose" (p. 198). Hanrieder appears to expect something like a new nationalism, perhaps in the style of that presumptive coalition of Christian Social Union and National Democratic Party to which he alludes on p. 195. Yet political thunderstorms, we have learned, often come from a direction quite different from the expected one.

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American Imperialism in the Image of Peer Gynt, Memoirs of a Professor-Bureaucrat. By E. A. J. Johnson. (Minneapolis: Univ. of Minnesota Press, 1971. Pp. 336. $12.75)

This intellectual autobiography is more memoir than interpretation. It summarizes the public experiences of a sensitive, imaginative, intelligent, and highly literate scholar with a tough will and a searching mind. Its title and its first chapter announce its intention to provide a general critique of American foreign policy in its current imperialist mood. But while its criticisms are accurate and cumulative, they are also fragmentary and impressionistic.

The very disingenuousness of Professor Johnson's theme makes a greater impact than do many more pretentious efforts at explaining and reconstructing American foreign policy. Johnson explains American imperialism as neither a coherent misguided theory nor a vile conspiracy against weaker peoples, but as the work of bunglers, incompetents, and minor sinners. American imperialism (which is nowhere defined in the book) appears as an accident of power. In this context Vietnam becomes a venal sin, not an unjust war. If the same sinners responsible for Vietnam also successfully reconstructed Europe and salvaged Korea, it was because America's intention is generally to do good, and because some good technicians and administrators are able to rise above the weaknesses of casual policies, interfering interests, and foolish organization styles.

This view of American foreign policies scarcely permits the emergence of much moral indignation: that passion Johnson reserves for the Nazis, Senator Joe McCarthy, and a few small-minded bureaucrats like the military authorities who denied him the Legion of Merit and gave him the Bronze Star instead (page 127), and the Yugoslav officials who kept hedging about a house their government was supposed to have provided for him when he was assigned to the AID mission there (pp. 249–50). Syngman Rhee earns contempt for his grand and petty thieveries, and so does the Cosmos Club for denying membership to Carl T. Rowen. Some of Johnson's recommendations were stupidly overruled or ignored, and this fact, too, is the occasion for indignation:

his strong position against letting the free market allocate rice during an anticipated food shortage in Korea and his efforts to reduce the swollen and indolent American mission to Greece in 1961 in order to supplement its hard-pressed and overworked counterpart in Yugoslavia, for example. He still resents being forced to "doctor" some statistics in order to justify an otherwise unjustifiably large shipment of wheat to Yugoslavia in 1952, merely to satisfy an insistent farm lobby. His scorn for the incompetencies and stupidities of American decision making in foreign policy is still fresh: for the stop-and-start funding processes that prevent programs from developing momentum and coherence (pp. 195–96), and for the powerful bureaucratic rivalries and disagreements that studded the decisions that came his way (pp. 223–24).

But this scorn is not Olympian. Johnson is not a raging Timon of Athens; his ambitions and perceptions are not Faustian; he sees not Machiavelli but Ichabod Crane at the tiller of U.S. foreign policy; the American presence in world affairs is Innocents Abroad, not conquering Caesar. Indeed, the Peer Gynt that Johnson sees is Ibsen's pathetic figure, not Grieg's brooding one. American imperialism has the soul of a button; it is not a "sinner in the larger sense," it is a "tragic non-heroic hero," a "clump of nothing" that "could not endure to have his ego 'shrunk to scale.'" Its unrest is that of a wanderer of the earth "trying to discover what it is he ought to be or do." Heedless, unguided and untrained, without great purpose or malice, then, America let a succession of events that accompanied power grow into an inadvertent imperialism culminating in Vietnam, a venture which is mentioned only once in the book (where it is described with the adjective "unfortunate").

The examples specified as "Gyntism" make it clear that though the offenses may be small, the consequences can be great: allowing vengeful thinking to interfere with necessary planning for the feeding and reconstruction of Germany; greedy, ignorant, and opinionated American military government officers who condoned involuntary sabotage of political reconstruction after the War (page 147, 167); corrupt overpaying of American contract advisers that only swelled the American presence abroad (p. 187); the mindless compromises that nullified principle (p. 158); individual cowardices and defalcations of duty (p. 216); and the repeated deceptions and concealments about the role of the U.S. government in postwar benevolent activities, especially in education, that undermined the credentials of foundations and private scholars everywhere (p. 286); together with the racism even of liberal elites, as revealed in the Cosmos Club incident, that discredited the American intentions everywhere in the Third World (page 304). "Gyntism" is not merely an American syndrome, of course: the French were also guilty of it in their wartime collaboration and their postwar confusion about punishing it (p. 138). Indeed, it is scarcely a syndrome at all: it is merely human weakness.

Is it enough to explain the ambitions and shortcomings of American foreign policy as human weakness? Within the limitations of human capacity among policy makers, are there not possibilities for an institutional rationality that will offset collective inadvertence and smallmindedness? Or, if even posing such a possibility demands too much of any Peer Gynt, can we not assess the causes of what psychologists used to call ineffective behavior? Stanley Hoffmann, in *Gulliver's Troubles,* defines similar facets of American character systematically enough to permit us to distinguish among the varieties of Gyntism of different nations. Many scholars of foreign policy have suggested how the processes of decision making, the perceptions of decision makers, and the coexistence of incompatible political forces, can explain some of the political aspects of foreign policy. In recent years, efforts to explore the bases of individual and social morality have applied psychoanalysis and philosophy to the problems of international aggression and misunderstandings. Quantitative analysis of both conflict and peaceful advantage seeking has also helped explain international events. But it is doubtful if an individual's memoir, however thoughtful and eloquent (this is both), however strategically based (this is an overseas view of administrative problems and achievements, and thus a useful counterweight to the Olympian view from Washington) can provide enough leverage upon experience to reach these hard questions. Nor can Mr. Johnson's professional skill provide the means of surmounting his data (he is an economic historian so dedicated to his craft that when he describes his boyhood chores on a farm in Illinois he justifies his immigrant parents in a footnote citing a study of child labor in Sweden).

The memoirs are an important source-book for a generation that did not experience the excitement and purpose of the postwar reconstruction and the first decade of foreign aid. It contains compelling vignettes of the military government and of the confident spread of faith in U.S. technological and economic capacity for modernization. It suggests the sense of self-fulfillment the American scholars in the postwar world have experienced when their advice and skills were sought and accepted, and it reveals as well their weaknesses as decision makers and administrators. Moreover, in the end Professor Johnson is probably right: there was more good than evil intention in these postwar manic decades of American activity. America's failures partake of the tragedy of mediocrity more than that of malevolence. Peer Gynt is an apt simile. But in this book it is only a simile.

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Europe's Would-Be Polity: Patterns of Change in the European Community. By Leon N. Lindberg and Stuart A. Scheingold. (Englewood Cliffs, N.J.: Prentice-Hall, 1970. Pp. vi, 314. $7.95 cloth, $5.50 paper.)

Political and economic developments in Western Europe which have been subsumed under the rubric of regional integration are among the most thoroughly researched and intensively analyzed of postwar phenomena. Responding to this profusion, Lindberg and Scheingold offer "not simply a book about the European Community, but also a book about theories and theorists of European integration and of international integration processes more generally" (p. v). The authors also choose to shoot at a moving target—to delineate patterns of change upon which to base projections about the Community's future. This ambitious endeavor, which they hope will meet "a need . . . for a theoretical and conceptual framework capable of providing an overall perspective on the often conflicting theories, descriptions, predictions, and evaluations" (p. v) of the European system, has produced a mixed result.

To this reviewer, it does not offer a fully satisfactory overall perspective (assuming one is needed), nor does the model of systemic change seem to overcome its tautologies sufficiently to afford a useful conceptual tool for future prediction. The Preface recites that the authors will construct "some analytical models to help us understand how and why changes in this political system occur." When the key model of system change is later presented, the reader is told that it "says nothing about how or when the posited changes might occur (i.e., the conditions and circumstances that cause the individual variables to vary)" (p. 115). Well, one might ask, why not? *Is* an equation which omits how and when changes might occur a model of change? The equation and its variables cannot be fully (or fairly) explored in a brief review, but a footnote explains that models are only steps along the road to theory, and simply consist of "abstract statements that posit relationships between variables that may or may not conform to reality" (p. 106).

A more significant dimension of *Europe's Would-Be Polity* is its contribution to an understanding of the integration process. In the postwar years, the problem of whether to rebuild or transcend the European nation-state was resolved by a compromise based upon the common commitment to economic stability and prosperity. The Community as it evolved in the 1950s and 1960s and remains today, the authors properly stress, has largely been the result of political and technical elites and is still largely economic and "consequently rather obscure" (p. 22). What has emerged, they state, is a would-be polity. To be sure, the nation-state is still predominant, but "its almost total monopoly has been clearly broken" (p. 75), and it has managed to cope with the evolution of the Community system in such fashion as to allow integration to proceed in economic matters while reserving to itself such domains as military security policy.

For Lindberg and Scheingold, political integration is defined by the scope and institutional capacity for decision making. Measured against its own record since it began, the European Community *has* achieved a significant level of political integration, and the authors look to the patterns in the past ups and downs of the Community to build their alternative models of system change for purposes of future projections. The decisions which produce change are the outcome of a process of coalition-formation, whose mechanisms include functional spillover, side-payments and logrolling, actor socialization, and feedback. Related elements include demand flow, systemic support, interest-group pressures, the style and power of political actors, and the amount and kind of leadership available. A number of "alternative process models" are offered for purposes of analyzing systemic change, and detailed case studies are then presented to illustrate how the European Community experience has included such models as forward linkage (agriculture), output failure (transport), equilibrium (the general customs union), spillback (the coal sector), and systems transformation (successful with the birth of the common market, unsuccessful with respect to British entry in the 1960s).

The tendency toward tautology seems to re-appear in the case studies. For example (and in contrast to the case of agriculture), the inability to achieve a common transport policy is traced to poor Commission leadership and to a demand flow inadequate to generate the mechanisms of coalition formation, the latter because governments and relevant political actors did not so perceive their interests that a common policy was possible. In this case study, the authors emphasize the failure of the Commission to use the potential which (in their view) *was* available to build a coalition for a common policy, however modest. The Community's pattern of success and failure seems to be explained as follows: if there is sufficient demand flow and leadership, coalition formation and a common policy will result (agriculture); but if both are lacking, there will be failure (transport). Here again one might ask, Is this a meaningful insight or a not very theoretical runaround? And even if this explanation is not tautological, does it permit projections concerning *future* systemic change?

Lindberg and Scheingold conclude with an assessment, based upon their analytical framework, of prospects for continued Community growth. They admit that there is no practical way to use their model to predict numerous possible influences (such as international developments, unforeseen crises, or changes in government or ruling elites) which might introduce fundamental changes. Hence either forward linkage or spillback is equally possible, depending upon circumstances. We are given a European Community which will continue to grow in the short run, but which will gradually reach an equilibrium—based upon coordination and harmonization of *national* policies more than upon a common Community policy process. Future leadership, systemic support, the flow of demands, and institutional capacity are unlikely to change in such fashion as to trigger striking growth. The authors persuasively argue that neofunctionalism has just about reached its limits, rejecting the spillover theory and arguing that initial satisfaction with systemic growth leads not to new demands for broader decision making but to a conservative concern for protecting what has been achieved, i.e., equilibrium. "Ironically, then, the success of integration tends to retard growth" (p. 292).

Although in the end the authors admit how limited is their "ability to make exact projections about the Community's future" (p. 303), they are quite certain that it will stop growing "well short of the point where we can talk of federal or pseudo-federal structures" (p. 305). What, then, are we left with? Lindberg and Scheingold conclude that we shall be faced with "an unprecedented, but curiously ambiguous 'pluralistic' system. . . . Indeed, there seem to be no satisfactory models or concepts in the social science vocabulary to adequately describe it" (p. 306).

Thus, with respect to the future, *Europe's Would-Be Polity* does not present more than a clouded crystal ball. Indeed, since the authors wrote we have witnessed what may be a remarkable movement toward an expanded Community, with a projected economic and monetary union by 1980, an agreement on the desirability of British entry and further expansion of membership, and even a rudimentary step toward political union with the inauguration of periodic meetings looking toward policy cooperation in foreign affairs. Whether strengthening and expanding the Community can simultaneously be achieved remains to be seen; what is genuinely impressive in terms of traditional international relations is not that European integration does not proceed quickly, but that it proceeds at all. Even if a United States of Europe should not even remotely evolve, the Community *is* (as a member of the Commission recently observed) "an entity in its own right, not merely in constitutional terms, but also with respect to its position in world politics." Lindberg and Scheingold do not satisfy the desire for predictive powers (could anyone?), but they do demonstrate that a system which has experienced as many patterns of change as has the European Community must be at least a would-be polity, if not a real one.

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India's China War. By Neville Maxwell. (London: Jonathan Cape, and New York: Pantheon Books, Random House, 1970. Pp. 475. $10.00.)

India inherited her ill-defined northern borders with China from the British Raj, and attempted to deal with them in the traditional British manner. The policies failed because India lacked Britain's resources and was confronted with a different kind of China.

Duncan Sandys, who headed a British mission to India in November 1962, was reported by Galbraith (*Ambassador's Diary*, p. 495) to believe that "the whole argument with China [was] over a few acres of Ladakh which he would have the Indians surrender." Had India been willing to negotiate the conflicting border claims, would Peking have agreed to yield NEFA in return for the Aksai Chin ("desert of white stones") area, and the trauma of the 1962 war been avoided? Whereas India's strategic interest in NEFA is indisputable, it is difficult to discern any tangible value in her possession of Aksai Chin.

In his comprehensive examination of these disputes, Neville Maxwell provides the basis for an informed answer to this and related questions. Posted to India in 1959 as the South Asia correspondent of *The Times,* he found that for the next three years the dispute was the "staple of my work," and a continuing interest until his departure in 1967, as well as subsequently at the London School of Oriental and African Affairs. He draws heavily upon secondary works for historical background. For the contemporary period he relies upon readily available documents and others that remain classified, as well as upon numerous interviews with both Indian civilian and military personnel who were intimately involved. Although the book is formidably "backnoted"—899 times to be exact—many of his sources for recent information are understandably held in confidence. Aside from the irritating necessity of turning to the back of the book for the references, and the virtual impossibility of discovering the referents of many of his numerous *"op. cit.'s,"* the book is remarkably free of similar defects.

Originally intended as a rewrite of his dispatches to be included as a section of a book on India in the 1960s, the project, Maxwell soon realized, required "something much more full, fundamental and searching. . . ." *India's China War* is the product of his "subsequent reappraisal" (p. 12). After an "Historical Introduction: The Limits of Empire," (45 pages) the story unfolds in five generally chronological chapters: "Collision Course" (105 pages); "The Forward Policy" (86 pages); "The View From Peking" (33 pages); "The Border War" (120 pages); and "Ceasefire" (30 pages).

Had the author set out to vindicate the Chinese to the disadvantage and discomfiture of Indians he could not have done a better job. Yet one has the distinct feeling that this was not his intent, that his reappraisal led him, perhaps with some reluctance, to tell it as he believed it honestly was. The account is substantially as follows: Prior to 1947, none of the contested areas had been delimited by valid international agreements. Of some eleven different lines drawn by the British in the Western Sector, only one had even been proposed to China. The MacMahon Line in NEFA, never agreed to, and in fact specifically repudiated by all Chinese governments, had been approved by Tibet, but only on the understanding that it was part of an arrangement, never fulfilled by the British, by which the latter would obtain Chinese consent to virtual Tibetan autonomy. Needless to say, the Tibetan right to negotiate an international agreement was at best dubious. Both areas remained practically unoccupied (except by tribes in NEFA) until this century, when the British, and subsequently the Indian government, began to move into the NEFA area and (after 1950) the Chinese into Aksai Chin. Presented with a number of alternative boundaries for their Western claim line, the Indian Government chose the most ambitious of the former British lines, but one that seems to have been regarded by London principally as a bargaining point for possible future negotiations. It was not until 1957 that the Indian Government, through an article in the Chinese press, became aware that the Chinese had constructed a road through Aksai Chin.

India maintained that there was no border dispute because her claim was incontestably valid, and hence there was nothing to negotiate about. The Chinese, on the other hand, exuding sweet reasonableness, recognized the existence of a dispute and were willing to settle it through negotiations. Moreover, throughout the conflict, Peking indicated a willingness to agree to the Indian claim to NEFA. It was logical for the Indian government to conclude that the Chinese would also yield elsewhere.

> But, in fact, by ruling that the remaining stretch of the northern borders should be regarded as a "firm and definite" alignment, "not open to discussion," Nehru had taken the step which was to transmute a boundary problem into a dispute, and the dispute ultimately into a border war (p. 81).

Perhaps it is open to refutation, but Maxwell presents strong evidence that the war was unnecessary, that the responsibility was primarily India's, and that had the issue been negotiated before it became a matter of national pride and honor, India would have gained the NEFA area and lost Aksai Chin, the latter of no tangible value to her.

The account of the Indian domestic scene, of the roles of Nehru, of Menon, and of Major-General Kaul and his "Kaul boys," is fascinating to one interested in Indian personalities and politics. So is the treatment of the strategy and tactics of the war which, despite the sad performance of the Indian forces, offered instances of courage, skill, and wisdom. Perhaps the one benefit that India derived from the conflict was a more realistic assessment of her military forces and the improvement that took place prior to the 1965 conflict with Pakistan.

Those interested will find perhaps the best informed of the Indian reviews of this book (unanimously unfavorable) in K. Subramanyan, "Neville Maxwell's War," *Hindustan Times,* October 18 and 25, 1950; the review also appears in the *Journal of the Institute of Defense Studies and Analysis,* III (October 1970), 268-91. Those firmly convinced and unwilling to consider any other possibility than that India was the innocent victim of an unprovoked Chinese attack should avoid this book.

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The Art of the Possible: Diplomatic Alternatives in the Middle East. By Michael Reisman. (Princeton, N.J.: Princeton University Press, 1970. Pp. 161. $6.00.)

This book is an exhortation and a proposal. It exhorts the United States and the Soviet Union to cooperate in preventing the Arab-Israeli conflict from setting off a world conflagration. The proposal is for arrangements establishing "minimum order" in Arab-Israeli relations. The assumption underlying the proposals is that at present "peace" between the Arab states and Israel is unattainable. Instead, diplomacy should aim at the more modest and more realistic goal of "minimum order."

The arrangements proposed by the author amount to a comprehensive territorial settlement, with built-in guarantees in the form of international participation in machineries and institutions designed to separate the parties. The scheme calls for the return of Sinai to Egyptian sovereignty, accompanied by neutralization through the establishment of an internationally administered Sinai Development Trust. The future of the West Bank is to be decided by a series of referenda, which would provide an opportunity for the Palestinians to exercise the right of self-determination. The Golan heights would be set up as a Druze trust territory, administered by Israel under UN auspices. Jerusalem is to remain under nominal Israeli sovereignty, with a system of effective international guarantees for the rights of different religious communities.

The objections likely to be raised against these proposals, stemming from the interests of the parties concerned, need not concern us here. The proposals ought, however, to be evaluated in the light of the goal and principal criterion which the author has set himself: the goal is "minimum order," and the main criterion to guide the design is realism.

This reviewer tends to agree with the author that "peace," in the sense of normal neighborly relations between the Arab states and Israel, is at present unattainable, and that the more immediate diplomatic objective ought to be to diminish the likelihood of renewed conflict. The problem is how can this be achieved, and how can processes be set in motion which would tend to reinforce and stabilize a system of minimum order.

The system of minimum order outlined in this book is to be brought about by the cooperative efforts of the United States and the Soviet Union. In setting the background for this cooperation the author draws an oversimplified symmetric relationship between the Soviet Union and Egypt on the one hand and the United States and Israel on the other. He lists some important common interests shared by the United States and the Soviet Union, and suggests that these can be made to prevail over their competitive interests. The Arab states and Israel are assigned the rôle of reluctant clients, who will behave in accordance with the guidelines laid down by their respective patrons. The entire scheme depends not merely on a momentary Soviet-American agreement on the terms for a settlement, but on continued concert between them, and a long-term insulation of the Middle East from competitive power politics. I wonder whether this accords with the "realistic" and "achievable" goals that the author says are required.

Another weakness of the scheme is that in at least two out of the four major proposals, the argument seems to be inconsistent with the purpose of minimum order. One concerns the West Bank. After presenting a good argument for permitting the Palestinians on moral grounds to exercise self-determination, the author continues by discussing the security advantages of his proposal. Here his argument is that if commando activity by West Bank Arabs against Israel continues, "the existence of a Palestinian state from which attacks originated would provide a legitimate target and would render the threat of Israeli retaliations an effective deterrent . . ." (p. 52). One is left to wonder whether a "minimum order" advocated in these terms would constitute an improvement over the past and the present.

A more fundamental internal contradiction is raised by the author's proposal for a Druze trust territory in the Golan. The author implicitly acknowledges that for the time being the Druzes have not given rise to a movement claiming self-determination. The author says that "if our era is indeed characterized by the demand for political self-determination of discrete groups, then it is predictable that the Druze, at some point in the future, will begin to make such a claim" (p. 65). This hardly is a good reason, however, for inducing such a movement among them—as this is the effect that the establishment of a Druze trust territory would undoubtedly produce. Nor is it clear how the arousal of Druze irredentism in Israel, Syria and Lebanon, where none exists for the time being, would promote the goal of regional order and stability which the proposal is designed to serve. Indeed, the author's argument on this proposal seems to go against his own views of the preconditions for peace, as expounded in Chapter VI of the book. In that concluding chapter of the book the author laments that "no leader in the Middle East has been able to rise above his ethnicism, tribalism, nationalism . . ." and states that "the real problem in the Middle East is neither territorial nor ideological; it is the continuous inculcation and reinforcement of parochial subjectivities that cannot tolerate coexistence with others" (pp. 86–87). The stimulation of Druze political demands is not likely to facilitate the transformation of values and disappearance of nationalistic attitudes which the author deems desirable.

There is merit in the author's argument that such highly intractable problems as the Arab-Israeli conflict indicate "the need for social and political invention" and for "an unfettered investigation of new political techniques and legal institutions, tooled to goals of minimum order and the particular exigencies of the context in which they must be realized." Had the author undertaken a systematic investigation such as he recommends, the book might have advanced this purpose.

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Nullity and Revision. The Review and Enforcement of International Judgments and Awards. By W. Michael Reisman. (New Haven and London: Yale University Press, 1971. Pages xvi, 900. $25.00.)

Employing the policy-oriented perspective and the analytical framework developed by Professors Myres McDougal and Harold Lasswell, Dr. Reisman has examined in exhaustive detail the problems of revising, nullifying, or giving effect to the awards of international arbitral tribunals and the orders and judgments of international courts. That part of his monograph concerned with the doctrinal debate on finality or reviewability of arbitral awards, and with claims of nullity made with respect to arbitral agreements and awards, was originally presented in 1965 as a doctoral dissertation at the Yale Law School. Two additional years were then spent in expanding the study into its present comprehensive form. It now includes three major parts.

Part I, "The Anatomy of Authority," is a theoretical study of the relationships of authority (defined as shared group values which provide standards) and control (i.e., devices available to secure desired behavior in others) as they affect the reception of international arbitrations and adjudications. Reisman rejects the artificial model of the past which posited the finality of awards and judgments on the basis of prior acceptance by the parties of an obligation to be bound by them. He presents abundant evidence of a far more complex and equivocal set of relationships running through the processes of compromissary agreement, the arbitral or judicial process (including definition of relationships between state claimants and injured nationals for whom they stand as surrogates, characteristics of the several types of tribunals, here labeled "cogentive," "dispositive," and *ad hoc*, and their procedures), and the review of awards and judgments. Review is to be understood not merely in the sense of a judicial reopening or reconsideration of cases but of the broader political-social processes which determine how far the judicial decisions become effective (in truly Lasswellian diction, ". . . who, with what objectives, with what means at their disposal, how manipulated, where and with what effects?" [p. 15]). As he observes, international awards and judgments frequently only mark the beginning of renewed diplomatic and political effort to achieve a modified basis of settlement. There has been little enough realistic study in such terms of the extent of political-social acceptance of judgments of national courts. In the international arena the problem is even more complicated. The international community usually lacks institutions for direct review and enforcement, although it may be able to muster indirect controls reflecting a sense of the requirements of "minimum world public order." It cannot avoid reopening the tribunals' work to scrutiny by each national community affected, with all that implies of competitive diplomatic and political forces. Reisman develops, on the basis of much historical evidence from past cases, heuristic models of the sequence of procedures from *compromis* through arbitration or adjudication to the steps taken to give effect to or to modify or annul the award or judgment. These he hopes may assist the international decision makers to assess more realistically at each stage the probabilities of effectiveness and to manipulate controls available to them so as to contribute to it.

His analysis of these processes is the most thorough and useful with which I am familiar, although it owes a good deal to some earlier writers, notably Carlston (a debt Reisman acknowledges). Its perspectives rest strongly in sociological and realist jurisprudence. Its materials include a wide range of arbitral and judicial opinions, diplomatic instruments, and doctrinal writing, which he has subjected to careful analysis and sharp judgments. Unfortunately, especially as some of the presentation is quite abstract, Reisman lacks a feeling for succinct style and plain, unpretentious language; the section is therefore laborious although profitable reading. Fortunately the remainder of the book achieves a somewhat simpler, more direct style.

Part II, "Claims of Nullity," consists of two long chapters which examine the grounds of nullity asserted with respect to compromissary agreements and arbitral procedures, respectively. The first necessarily opens a good deal of general treaty law, as it applies to the validity of submission to jurisdiction in terms of competency to negotiate or invoke state responsibility, coercion of parties, national requirements for ratification, domestic jurisdiction limitations, state succession to obligations, unilateral abrogation or other termination, interpretation of agreements with respect to exhaustion of local remedies or pendency of litigation elsewhere, estoppel, and other matters. With respect to grounds of nullity in the arbitral process, Reisman rejects *a priori* limitations upon the scope of review such as exclusion from it of "errors of fact" or those "errors of law" conceived not to be "essential error." Such limitations he considers inconsistent with the fundamental purpose in review of contributing to the effectiveness of the decision and maximizing its authority. He is critical of doctrinal literature favoring such limitations or attempting definition of them. Accordingly, he examines a broad range of grounds of nullity: these include competency and propriety of conduct of arbiters and judges as evident in the procedure of appointment, participation in hearings, consultations, and decisions, disqualification to avoid possible prejudice, and corruption; geographical factors affecting the capacity of parties to present evidence; crises which affect the acceptability of decisions; considerations of equity; the propriety of *non liquet* judgments to avoid unacceptable consequences of judicial legislation; failure to accord right of hearing and representation and to compel discovery of evidence by adverse parties; coercion especially as it affects exhaustion of local remedies. Reisman has ransacked the literature of international arbitration as a basis for discussion of these topics; his erudition is impressive. Without suggesting agreement with his critique of every case he discusses, I can testify to his incisive analytical powers and consistent effort to relate his analysis to the principles developed in Part I. This section is a substantial professional contribution of interest particularly to international lawyers.

Part III, "The Process of Enforcement," begins with a general discussion of principles which should govern enforcement of international awards and judgments. In the absence of an "international sheriff," Reisman looks to a varied "functional enforcement." This does not exclude such controls as use of force or mustering the moral suasion of the international community, but he makes little of these as practical resources for general use. He relies mainly upon greater ingenuity and flexibility in reaching substitute assets to satisfy awards. He reviews in detail enforcement theories and practices in international organizations: the League of Nations under Article 13(4) of the Covenant, with a useful review of the Optants Case; the United Nations, with consideration of possibilities under Article 94(2) of the Charter in the light of the problem of enforcing interim measures ordered by the International Court of Justice in the Anglo-Iranian Oil Case; the Specialized Agencies, with a suggestion that the International Banks and the Monetary Fund might control assets of depositor-states to satisfy judgments against them; and the regional organizations of Europe, Latin America, and Africa, which now differ greatly in the institutionalization of enforcement through national agencies. Reisman then moves to enforcement of international awards and judgments by national executive agencies and national courts. Here he examines the implications of the decision in the case of the *Monetary Gold Removed from Rome*, and reviews doctrinal literature and recent cases with respect to the impact of the doctrine of sovereign immunity upon enforcement by national courts of foreign awards. The section concludes with discussion of the circumstances in which national self-help may still be appropriate to vindicate international legal rights when other authority fails to secure them. Part III is certainly the most systematic, unified discussion of enforcement of international arbitral and judicial decisions available to us. While the relevant material is not novel, it is assembled and focused upon the problem in a manner not hitherto undertaken.

The discussion is valuable for students of both international law and international organization.

Dr. Reisman's research has been prodigious; his analysis is searching, and his conclusions throughout are persuasive and significant. His work merits widespread professional attention.

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The Law of Treaties, A Guide to the Legislative History of the Vienna Convention. By Shabtai Rosenne. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1970. Pp. 443. $25.00.)

This is not a treatise on the law of treaties. It is a documentary guide through the complex process which, between 1962 and 1969, produced the Vienna Convention. For the work of the International Law Commission on this project between 1949 and 1962, when J. L. Brierly, Sir Hersch Lauterpacht and Sir Gerald Fitzmaurice were its successive rapporteurs, the reader is referred to the 1962, 1963 and 1964 reports submitted to the Commission by Sir Humphrey Waldock, the final rapporteur.

For all purposes of academic research and governmental or judicial inquiry into the long series of debates, compromises, and votes by which the eighty-five articles of the Convention were hammered out, the guide is priceless. It is another major contribution to the study and development of international law by an author who combines dedicated scholarship with exceptional practical experience and who has been since 1962 an active participant in the work of codification.

In a 66-page introduction, Rosenne emphasizes the supreme importance of this part of the United Nations program for development and codification of the law of nations. What is involved is the law of international contract, and the law of contract is, as he puts it, "the very heart of any coherent legal system." The decision insisted upon by Sir Humphrey Waldock when he accepted the role of rapporteur was that the goal should be not a mere model code but a convention binding upon states. The decision meant that the rules to be formulated must be adaptable to the multiple purposes that treaties serve in the relations of states and to radical changes in the nature and scope of the problems to be met. This called for abstract drafting, which increased the importance of adequate provision for dispute settlement; failing this provision, abstraction could mean excessive subjectivity, not to say license, in interpretation and application. Of the two most stubborn political battles in the entire process of negotiation, one was that between advocates and opponents of compulsory arbitration or adjudication. The other was fought over the question of universal participation in the convention. How a group of African states worked out the "package deal" that saved the conference from shipwreck on these reefs is a stirring part of Rosenne's introduction.

The author takes an optimistic view of the culmination at Vienna. He concludes that "a project of codification, which the International Law Commission had initiated twenty years previously, was completed." How much does this mean in view of the fact that eighteen states, including the entire Communist bloc, abstained in the final vote, that France voted No, and that by December 31, 1970, only Canada, Jamaica, Nigeria, Syria, and Yugoslavia had ratified or acceded to the Convention? Can the task really be considered complete until the text has been generally ratified? Perhaps the author would say that, ratified or unratified, the Vienna Convention will stand as the supremely authoritative statement of the law of treaties and will be relied upon as such by judicial and arbitral tribunals. It has already been cited in the Advisory Opinion on Namibia handed down by the International Court of Justice on June 21, 1971.

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Stilwell and the American Experience in China, 1911–45. By Barbara W. Tuchman. (New York: Macmillan, 1971. Pp. xv, 621. $10.00.)

As commander of U.S. forces in China, Burma, and India and Allied Chief of Staff to Chiang Kai-shek from 1941 to 1944, Joseph Stilwell was the central American figure in the first phase of the tragic relations between the U.S. and China. This period has been treated by many authors in different ways. Theodore White's *The Stilwell Papers,* a best-seller, consists of selections from Stilwell's wartime diary. Subsequently, this period of Stilwell's career was covered wilh deep insight by Herbert Feis. It was examined authoritatively and with meticulous care by Charles Romanus and Riley Sunderland. An evaluation of Stilwell's programs and actions in the context of an interpretation of American policies and Chinese politics was attempted in the reviewer's *America's Failure in China.* In terms of methodology and historiography, this book represented a political scientist's self-conscious endeavor to give the central place to the explicit and systematic analysis of the assumptions and structure of policy in the writing of diplomatic history while using the traditional form of historical narrative as a supplement. But Stilwell as a person and his early life are not the subject matter of these works.

Barbara Tuchman's full-length biography fills this important gap. It is written with the style of a master of popular history and character sketches. It evokes once again strong emotions of sadness, an intense sense of despair, and an oppressive feeling of doom in those who have lived through or studied this period of history. It enables us to see how Stilwell's personality, his early experiences, beliefs, and likes and dislikes shaped his outlook and activities during the Pacific War.

The theme of Tuchman's book is "the relations of America to China, in a larger sense to Asia" and "the vehicle of the theme is the career of General Stilwell" (p. xi). Thus an interpretation of this relation is a major thread running through the whole book. During the Pacific War, the American approach to China is characterized by Tuchman as the "can do" spirit, using the motto of the 15th Infantry Regiment (stationed in Tientsin in the 'twenties) in which Stilwell served. In the 'fifties and early 'sixties, the writings on both current problems and failures of American policies during and after the war were also characterized by a search for more effective policies, approaches, and methods to use American power to effect American purposes in a "can do" or "could have done" spirit.

The Vietnamese war since 1965 has challenged this spirit and its underlying assumptions. It has raised three interrelated questions: First, can the U.S. ever understand the problems of Asian nations? Second, can American goals in Asia ever be attained? Third, can American policies and actions ever have any effect on the political development in Asia? Tuchman's book reflects this drastic change in perspectives, but she has not formulated her conclusions and assumptions as carefully as many other scholars who share these new perspectives.

With regard to the first question. Tuchman correctly rejects the previous naive tendency to believe that China can be understood in American terms and judged by American standards—a tendency which many of us have examined and criticized at length. Unfortunately, she swings in the opposite direction of the nineteenth-century obscurantist position that the East and West cannot really understand each other. It is one thing to affirm the importance of cultural differences. It is quite another thing to assume the validity of popular clichés about these differences, to use them to explain conflict, and to conclude that they prevented two nations from ever understanding each other.

Thus, Tuchman allows facile generalizations about cultural differences between the U.S. and China to mar her narrative on the conflict between Stilwell and Chiang Kai-shek at important junctures of her story. She refers to the "fundamental cultural clash between Chiang's and Stilwell's theories of war"; the different forms which conscience "stemming from Judaeo-Christian tradition" takes from that rooted in "the Confucian tradition"; the Chinese "cultural imperative" of hoarding resources; the Chinese tendency to manipulate foreigners; Chinese cynicism, etc. (pp. 250, 266, 276, 288, 303, 332, 412, 490).

Yet Tuchman herself and other scholars before her recognize that at stake in the irreconcilable conflicts of policies and interests between Chiang and Stilwell was nothing less than Chiang's dominant position and the future development of China (p. 502). On this crucial question at least, there was no mutual misunderstanding due to cultural differences. On the contrary, the intensity and irreconcilable nature of the conflict arose from fairly shrewd guesses about each other's attitudes and an almost perfect understanding of the consequences of the policies pursued by the other side.

It is also clear that the intense conflict between the U.S. and China arose not so much from conflicts of national interest in the abstract as from the actual and potential impact of American policies on China's internal politics. This is conclusively shown by the facts that Chiang's political rivals demonstrated great enthusiasm toward Stilwell's army reform program and that the Chinese Communists expressed a willingness to serve under Stilwell. In return, Stilwell entertained a high opinion of them, in contrast to his utter contempt for and hostility toward Chiang.

Stilwell's experience in China also suggests the decisive importance of internal politics in the relations between states in another way. If one side in the conflict succeeds in exploiting the internal divisions in the other side, winning over the most influential actors or helping build a triumphant coalition favorably inclined to its viewpoint, it will prevail. In his conflict with Chiang, Stilwell had the firm and consistent support of Marshall and Stimson. But Chiang won over many influential American officials to his side. Subject to conflicting advice and cross-pressures, Roosevelt never acted decisively either by backing Stilwell to the hilt or by transferring him on many occasions of serious conflicts prior to September 1944.

When his hand was forced by the military crisis in China in the Summer of 1944, Roosevelt adopted *in toto* the Stilwell-Marshall program, including a demand to put Stilwell in command of all Chinese forces. But he was never fully convinced of the correctness of his course of action. When he was confronted with Chiang's threat of a rupture of the alliance, and when urged on by Hurley, he backed down.

The history of Sino-American relations and Tuchman's latest account of them once again remind political scientists of a lack of an adequate theoretical model for the conceptualization of these relations. Recently, Graham Allison has developed three models of foreign policy as rational choices, outputs of organizations, and outcomes of bureaucratic bargaining. But even this sophisticated formulation is not complex and comprehensive enough to serve as theoretical models for the analysis of China's foreign policies in this period, unless we interpret his third model of bureaucratic bargaining very broadly and loosely. In China, foreign policies pursued or advocated were functions of political struggle among various independent or semi-independent political groups which, in contrast to those in Allison's model, did not occupy definite positions in a hierarchical system and did not bargain merely along regularized channels. The pace, structure, and rules of the game, if these existed at all, emerged out of the process of struggle itself and changed constantly. The power of the political groups came ultimately from their control over armed forces. Not only did their internal power position in a fluid political situation determine their views on foreign relations; relations with foreign powers also had direct effects on their internal power position. This type of foreign-policy making which has occurred throughout the ages has always been well known to diplomatic historians, but it is incumbent on political scientists to formulate a conceptual model for it. After the decisive factor of internal politics has been taken into account in the construction of the model, we can then proceed to construct a model of greater complexity by including the cultural variable.

With regard to the second question of whether American goals in China during the Second World War could have been achieved, Tuchman's answer is perceptive and has serious implications for current American policies toward Asia. Obviously influenced by the lack of decisive results of the massive American efforts in Vietnam, Tuchman has come to stress the limits of American power more heavily than many of us did at an earlier time. She believes that piecemeal reform was impossible and that even if the U.S. had adopted Stilwell's policy of making American aid conditional on Chinese performance, the result would still have been nil. "Stilwell exaggerated the power of the quid pro quo because he did not have it" (p. 308). But underneath, Stilwell knew that to reform the Chinese system, "it must be torn to pieces" (p. 381). Not surprisingly, Stilwell was one of the first to draw the correct conclusion when he wrote in August 19, 1945, five days after Japan's surrender, that "we ought to get out—*now*" (p. 522). But the whole history of American policy since 1941 shows that it has been easy to make commitments but extremely difficult to "get out." Today, "getting out" of the involvement in the Chinese Civil War would be even more difficult than at any juncture since 1945.

If Barbara Tuchman is correct in her conclusion that "China was a problem for which there was no American solution," her rhetoric has probably carried her too far when she writes that "in the end, China went her own way as if the American had never come" (p. 531). This answer to the third question posed above is less than adequate. It is easy to see that what the U.S. did or failed to do inevitably affected the external relations and internal development of China even if the results were not those intended by the U.S. When the U.S. failed either to force Chiang to undertake the necessary reforms or to discard him, when she failed either to intervene effectively or to withdraw completely from China, when she failed either to sustain the non-Communist forces in China or to reach a *modus vivendi* with Chinese Communists, and when she neutralized the Formosa Straits, crossed the 38th parallel, and rushed headlong to the Yalu River, she in effect guaranteed the emergence of a violently anti-American Communist regime in China, made it necessary for the Chinese to be dependent upon Moscow for a fairly long period of time, and contributed to the eventual destruction of the political influences of pro-Western, moderate, non-Communist Chinese. To affirm that the U.S. opted for the worst possible alternatives in the years between 1941 and 1950 does not imply that American policies did not limit China's options and China's own choices in both internal and external affairs.

Tuchman's conclusions and assumptions on America's relation to China and Asia reflect powerful intellectual currents released by

America's frustrations over the Vietnam War. It remains to be seen to what extent these intellectual currents will become the basis of official policies. Whether these assumptions and conclusions are correct or not, only another encounter with destiny will tell. But even then, the test will not be conclusive. If there are firm lessons which we can learn from the totally unexpected and tragic outcome of Sino-American relations in the past thirty years and the rapid and drastic shifts in our interpretations of that history, these are the uncertainty in human affairs, the unpredictability of historical development, and above all, the need for intellectual humility.

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Survey of International Affairs 1962. By D. C. Watt. Issued under the auspices of the Royal Institute of International Affairs. (London, New York, Toronto: Oxford University Press, 1970. Pp. 558. $14.50.)

Political scientists and international historians will long be indebted to the Royal Institute of International Affairs for sponsoring the series of annual surveys of international affairs and to D. C. Watt for composing the present volume which encompasses world events from varying dates in 1961 to the end of January 1963. The arduous task of transforming raw data into patterned history or political analysis is of course best realized if the working scholar is allowed to transgress the bounds of a short period so as to see events in the light of previous years, decades or centuries. In few fields is the time factor more crucial than in international relations. For the essence here is the continuous interpenetration of multiple foreign policies that are disparate not only because they emanate from different power centers and cultural traditions, but also because they are projected onto time in greatly various ways. Thus, whereas the process of foreign policy making has been essentially short-term in twentieth-century American history (a view receiving considerable support from Mr. Watt's presentation), that of the Soviet Union is cast in long-range terms, allowing for the protracted realization of certain basic and enduring concerns. And this means, of course, that the former can be integrated more persuasively in an annual survey than the latter.

In the face of these difficulties it is no small tribute to the scholarship and talents of Professor Watt that he was able not only to chronicle a year dense with internationally significant events but also to shape the fluid relations he records by isolating themes and locating major junctions between quite separate, often starkly conflicting policies. Furthermore, he has rendered his version of the interactions in eminently lucid, readable prose.

The core of the work is a thought-provoking, well-aimed, often scathing critique of the Kennedy Administration's "Grand Design" for the restructuring of the Atlantic Community. This part, together with the chapter on "The Financial Policies of the Western Powers" (written by Michael Donelan), deserves to be widely read especially in light of Britain's entry into the Common Market in 1971 and the continuing disarray of monetary policies. Conceived on the model of the Marshall Plan, which was "only imperfectly understood in the U.S." (p. 137), the Grand Design was doomed to fail according to Watt because it was built on certain purely hypothetical propositions, namely that Britain and other E.F.T.A. powers had in fact entered the Common Market, that the E.E.C. was about to transform itself into a political community, and that having done this, it would be content to be a junior non-nuclear partner, ready to leave the responsibility for its ultimate defence to the nuclear deterrent power of the U.S. (p. 112). Preoccupied with the military rather than the political uses of their armed forces, the Americans, notably the President himself, were in Watt's view so mesmerized by the fear of nuclear proliferation that they missed out on the vast task of transforming superficial contacts with their European allies into meaningful communications. Although the author notes that "negotiation was not President Kennedy's personal forte" (p. 13), he squarely identifies the major culprits in this sad story as the President's ideology-ridden advisors (with McNamara and Rostow singled out for special rebuke), who were ignorant of elementary diplomatic techniques, preoccupied with unfolding the "Design" in rhetoric rather than methodical planning, and allowed by their Chief to indulge in rival, mutually contradictory initiatives.

The major illustrations here cited are the failure to perceive the organic connection between policies toward the Alliance on the one hand and the crisis of confidence in the dollar on the other; the mishandling of the Skybolt missile project which revealed the Administration's "inability to reconcile the methods of its strategic and defence policy with the goals of its foreign policy" (p. 160), and the inept treatment of the arms control and disarmament proposals. A study of the latter, said to have included virtually every idea that had emerged from discussions of disarmament since the interwar years, leads Watt to a poignant critique of the "American penchant for mixing the legalistic with the idealistic in their approach to international affairs" (p. 26)—reaffirmed in a subsequent chapter by references to this country's emotional, populist, quasi-parliamentary view of the UN—and to the general conclusion that public understanding of the nature of negotiation had not improved much since 1929 when Lord Robert Cecil complained that American delegates come with precise instructions, usually made public before their arrival, and that any variation from plan is then likely to be regarded as a diplomatic defeat for the U.S.

The bias in some of these discussions is decidedly British, as when Watt writes (p. 122) that Britain should have been the interpreter of the new Europe—a view that lays the author open to the same charge of unrealistic hypothesizing that he levels against the Americans. For after noting that Macmillan harbored bitter anti-American sentiments while priding himself on his ability to manage the Americans (p. 164), that Canada and Britain were the weakest among America's allies in NATO (p. 61), and that Britain and the U.S. pursued quite different, indeed rival schemes of solving the financial crisis, Watt himself deplores the "disastrous misjudgment" of the British in the Skybolt affair, as well as the "unfortunate penchant" of his countrymen for seeking their own peace of mind by suggesting concessions on the part of those whom their government is pledged by treaty to aid (p. 173). Likewise he is himself highly critical of Britain's uninterrupted industrial succor (in the form mainly of Black Sea oil deliveries) to Cuba during January-August 1962, all the moreso as this did not net the U.K. any diplomatic advantage. Further, in regard to the Cuban situation he notes explicitly (p. 106) that Britain no less than France was preoccupied with tripping up her allies rather than her enemies.

Having followed these altogether convincing strictures on the subject of Anglo-American relations, one finds it difficult to acquiesce in the author's uncompromising indictment of France and Germany as sole perpetrators of all that went wrong in the year. For the major thesis of the book has it that 1962 ushered in *the* great Soviet offensive, targeted in particular upon Cuba and Berlin, and that it ended as a triumph for de Gaulle and a resounding defeat not only of Kennedy but also of Krushchev. It is here in the treatment of Communist strategy and continental Europe's reactions to Anglo-Saxon and Russian policies that the Survey's own Grand Design disintegrates. For one thing, Soviet policy does not lend itself to the kind of easy personalization possible in respect of the U.S. or France. After all, Krushchev himself had proclaimed long before 1962 that all Soviet leaders are Stalinists when it comes to foreign affairs. His verbal behavior vis-à-vis the Atlantic nations or Communist China may deserve such epithets as "lurid" and "wild" (it is even described as "Faustian") by comparison with the measured cadences of Heath or McNamara, but in the traditional terms of Leninist polemics it is more properly viewed as an exercise in purposeful statecraft. Next, in the context of the Soviet operational code which enjoins tactical withdrawals and shifts of offensive from one scene to the other it is doubtful, especially in light of what has happened in both Cuba and the Germanies in the last nine years, that Krushchev's Russia was really defeated. Watt himself points out that Krushchev was at that time planning a step-by-step exacerbation of the conflict centering on Germany, Berlin and "The Wall" so as eventually to crack the American president's nerve (p. 34), and also that Cuba may have been a means to force the U.S. to yield on Berlin. It is thus disconcerting to find him say: "As to the reasons for the Soviet obsession with Berlin they can only be the purest speculation" (p. 58), and to castigate Adenauer for provoking a well-meaning Krushchev who "hoped for a rapprochement with West Germany and resented Dr. Adenauer's reaction to the Cuban crisis" (p. 74).

Parts II and III of the work, consisting of separate essays by Richard Gott and John Major on what is summarily called "The Third World," are of uneven quality. The piece "China defeats India" (Gott) is noteworthy for its excellent use of contemporary news items and policy statements even though it was surely doubtful even then that "the losses to China's prestige were immeasurable" (p. 426). Another major chapter on "The Sub-Systems of the Third World" has only two entries, the break-up of what then existed formally as the "United Arab Republic" and Africa south of Sahara where the discussion deals almost exclusively with the Union of South Africa, Portugal and Southern Rhodesia. A very useful index concludes this richly informative volume.

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International Claims: Postwar French Practice. By Burns H. Weston. (New York: Syracuse University Press, 1971. Pp. 224. $10.75.)

It would be a distinct loss to the political science profession if Professor Weston's book were studied only by specialists in international law or comparative government and politics.

Issued as the ninth volume in the Procedural Aspects of International Law Series, it undertakes a comprehensive and systematic probe of eight claims commissions established by France since World War II to develop and administer policy for the distribution of lump sums received as compensation from Poland, Czechoslovakia, Hungary, Yugoslavia, Bulgaria, Romania, Egypt, and Cuba. The author's translation, examination, and comparison with U.S. and British practice of processes and actions of the claims commissions, together with interviews with government officials and private practitioners, provide the reader with rich and reliable data and insights. Professor Weston, who teaches at the University of Iowa Law School and serves on the Executive Council of the American Society of International Law, is neither neophyte nor dilettante as he strives to clarify our understanding of contemporary mechanisms and institutions for coping peacefully with nationalization and other takings of foreign business and property.

He is as concerned with process as with substance. For example, he is troubled by the aura of secrecy that surrounds the work of the French claims commissions, even though the details he discusses and criticizes offer something of a testament to the capacity of a resourceful scholar to pierce successfully the bureaucracy's penchant for secrecy. With regard to substantive policy, he is especially critical of overemphasis by the commissions on recovery of allegedly lost profits. He finds scant support in historic practice for such dedication to protection of evanescent profit interests. He regards initial expectations of profit as a relevant item for inclusion in a list of claims but rejects payment by the commissions of estimated lost profits "as a matter of right" (pp. 113–114).

Professor Weston is equally critical of the circumscription of redress for claimants wrought by embodiment in the claims system of the simplistic adage that "without legal capacity to play the benefactor, no decision maker can be expected to give satisfaction" (p. 152). He finds that claimants have often been denied commission benefits simply because the controlling texts governing the commissions' powers have made distribution impossible. Sometimes settlement agreements failed to authorize indemnification for claims raised, and at other times, as with debt claims brought to the Bulgarian and Romanian commissions, the French Government chose to exercise its "diplomatically prescribed prerogative to distribute the lump sums according to its own dictates" (p. 153). Given the reasonable assumption that many claimants would not have undertaken to petition the commissions at all had they been aware of such limits, "one must wonder about the adequacy of notice on the part of the French government" (p. 154).

Professor Weston's volume is as rewarding for the general political scientist as it is for the international law or comparative politics specialist; for his analysis of claims practice eschews narrow or purely legalistic components in favor of a sophisticated examination of the multiple interrelated contexts in which claims are asserted. In probing commission processes and practices, he traces the successes and tribulations of a modern instrument of conflict resolution. The international claims commissions of yore having become museum pieces, the period since World War II has had as its hallmark for the peaceful resolution of claims resort to the lump-sum settlement/national claims commission device.

The international legal order today, as in the past, has a primarily horizontal structure. It has minimal hierarchical command and enforcement structures. Accordingly, Professor Weston believes that

> however much traditional notions about sovereignty and nationalism may be on the wane, the nation-state remains the primary repository of international legal authority and control, with most effective decisions being taken on a government-to-government plane among formally separate and equal bodies-politic (p. 1).

This recognition of reality should not, however, generate a self-fulfilling cynicism about international law. The author remains convinced that international legal significance can be distilled from practices of domestic decision-making institutions. Horizontal decision making can and does have "very real legitimacy . . . for the maintenance and growth of world public order" (p. 2). Although he relies primarily on international lawyers to implement his call for "energizing the horizontal possibilities for assuring and expanding world order" (p. 190), Professor Weston's articulate faith and meticulous scholarship deserve a wide audience among political scientists.

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The Politics of Trade Negotiations between Africa and the European Economic Community—The Weak Confront the Strong.

I. William Zartman. (Princeton, N.J.: Princeton University Press, 1971. Pp. xi, 243. $8.50.)

This pioneering book will interest students of African and European diplomacy as well as those primarily concerned with theories about the process of bargaining and negotiations. The author has undertaken a difficult comparative study of several sets of trade and aid negotiations among the six members of the European Economic Community on the one hand, and four groups of African states on the other: the eighteen African states associated with the EEC; the three East African states of Kenya, Uganda and Tanzania; the three Maghreb states of Morocco, Algeria and Tunisia; and Nigeria.

Although the book deals with much economic material it is essentially a political analysis of how "the weak confront the strong." The author emphasizes the process of negotiations rather than its results. He contends that African states have a kind of power that enables them to win benefits despite their "weakness," but he tries to avoid overstating the case. He also endeavors to make a contribution to the development of a "comprehensive theory of negotiations" but recognizes that his contributions have not led very far toward this end.

After a background chapter on the Treaty of Rome, four chapters tell the story of the disorderly and complex offers and counteroffers the European and African states made to each other in negotiating the Yaoundé Convention between the Six and the Eighteen, and the related treaties between the Six and seven other African states. Each of these four narratives concludes wilh a helpful "analysis" in which the author sets forth the conclusions he draws from the relevant negotiations. He suggests that in negotiating the Yaoundé Convention, "the African states greatest strength was their weakness" (p. 64). Their underdevelopment was the basis for their appeals as well as their excuse for not offering greater concessions. By putting their fate in the hands of the Six, where it was anyway, they increased their chances of achieving more than if they had tried hard bargaining when they had "nothing to bargain with." As this chapter also shows, the Six were so severely divided among themselves that when they did reach agreement in their own separate negotiations, they presented a tight package to the Africans, who had only the options of accepting the package or rejecting everything. France's hard line on maintaining its position in its former territories caused most of the friction.

The Africans, unfortunately, were also often divided, thereby weakening their position further. The Eighteen tried to focus on principles rather than details, but the Six soon "turned to working out the details among themselves, thus letting the details determine the principles rather than the reverse" (p. 62). The Eighteen could only warn of the dire results if they were left impoverished, and attempted to enforce Europe's obligations by repeatedly citing the voluntary commitments of the Six to continue the Association.

For both economic and political reasons, Nigeria had more success than other African states in negotiating with the Six. It was a potentially large market for both trade and investment, and it would be a valuable symbolic Associate since it was not only Africa's largest state but was politically moderate. In addition, Nigerian negotiators were skillful, and they had the advantage of acting alone rather than as part of a divided group of states. The East African states, on the other hand, had a much smaller appeal as a market and in the end could only take what was given them.

The Maghreb negotiations took six years (1963–1969) before even limited agreement was reached. The situation of these Maghreb states—Morocco, Algeria, and Tunisia—was more complex in one sense because they were on the Mediterranean, the southern flank of the Community itself, and their products competed directly with the produce of some of the Six. Italy, for example, insisted on excluding fruit, vegetables, and olive oil from the negotiations until the Common Agricultural Policy of the Six had been decided; Italy also required special treatment for cork. The position of Morocco, Algeria, and Tunisia was also weakened by their failure to negotiate jointly, a point the author elaborates at some length.

The fourth of the author's case studies of negotiations deals with the operations of the "Eurafrican" institutions set up by the Yaoundé Convention to deal with such matters as the problem of "origins," which, incidentally, the author does not explain as clearly as he might have. The chapter also covers the negotiations leading to the renewal of the Yaoundé Convention, signed on July 29, 1969. Though generally satisfied with the aid aspects of Association, Africans expressed many and repeated grievances over its trade aspects. The new Convention only "smoothed out the details" of the old one and left Africans dissatisfied. It produced "neither a vertical integration, nor a Commonwealth-Yaoundé African Association, nor a continental or subcontinental free trade zone, nor an effective price and market guarantee, nor a working marketing organization, nor even a successful preferential area" (p. 197).

The author's final chapter is a contribution to theory. Distinguishing between the study of diplomacy and the analysis of negotiations, he notes that the great writers on diplomacy—Thucydides, Machiavelli, de Félice, de Gallières, Pecquet, Nicholson—simply suggested how diplomats should behave, while the diplomatic historians only described this behavior in specific cases. In Zartman's view, even contemporary students of diplomacy (Morgenthau, Aron, Lall, Ikle, Fisher) have also sought to establish, or confirm by new methods, prescriptive principles for successful diplomacy. This "principles approach," whether it be power principles or behavioral principles, "provides useful lessons for diplomats, but little in the way of analysis of the negotiating process (how outcomes are attained)" (p. 202).

The author also believes that recent efforts to analyze bargaining through game theory and matrix methodology are inadequate for the analysis of negotiations because, *inter alia,* many positions in most negotiations cannot be expressed as quantifiable items. Game theory can help to determine whether to negotiate or not, but it is not applicable to the analysis of the process.

Zartman suggests that the main question in analyzing negotiations is, "How are divergent viewpoints combined to produce a common agreement?" (p. 222). His answer is what he terms the "alternative/convergence approach to the study of negotiations." Divergent positions may be combined (1) by limiting alternatives in some of four different ways, and (2) through the convergence of positions by five possible means. The details of this are too complex for elaboration here.

The author concludes with a brief application of his theory to these Eurafrican negotiations. In summary, he tells us that weak states can make the most of their weakness by effectively provoking encounters, by putting forth their needs, and by the very act of accepting what is offered them. The results achieved by the weak are thus quite limited, which may help to explain why the author focuses primarily on the analysis of the negotiations process rather than on the evaluation of its results.

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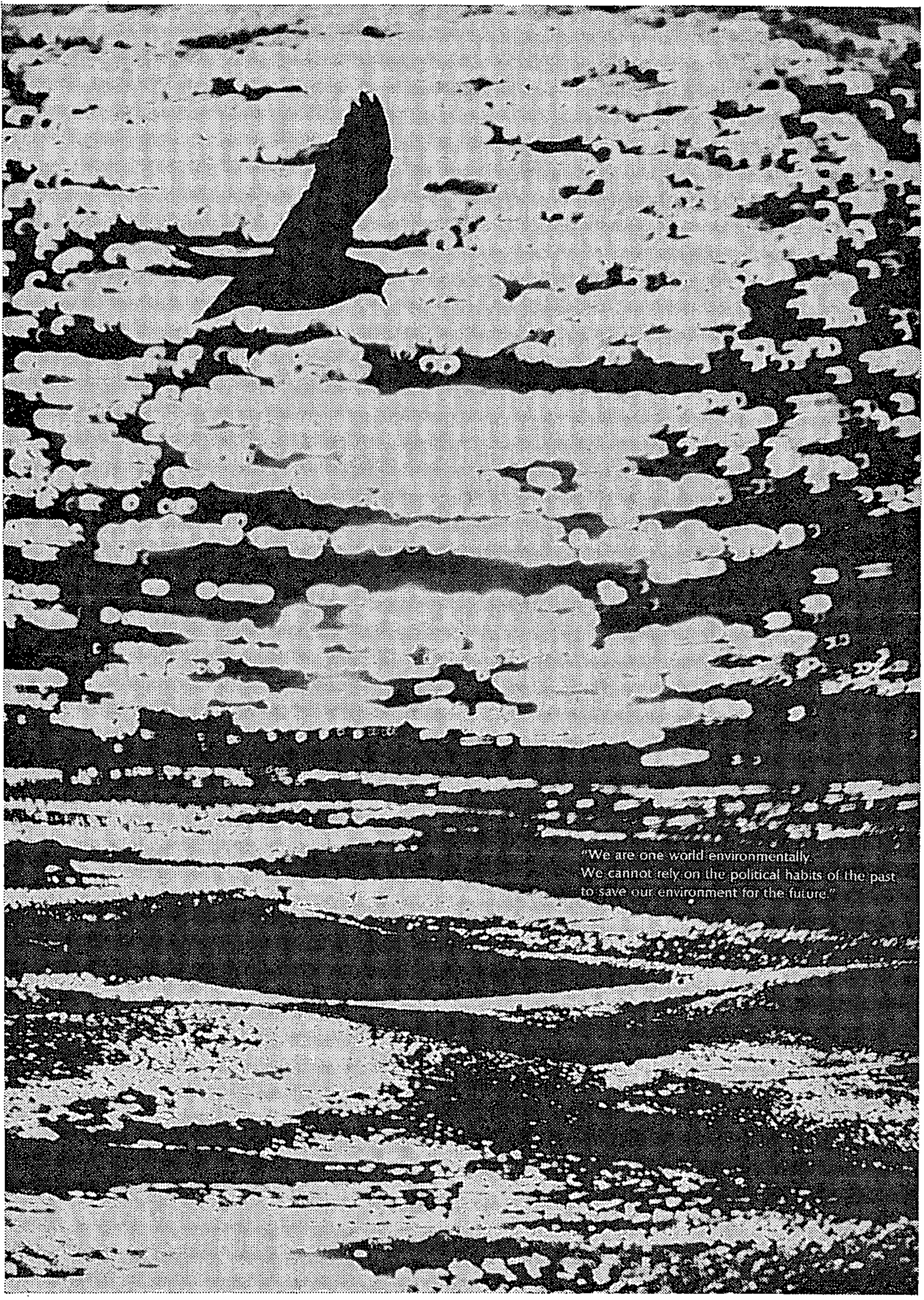
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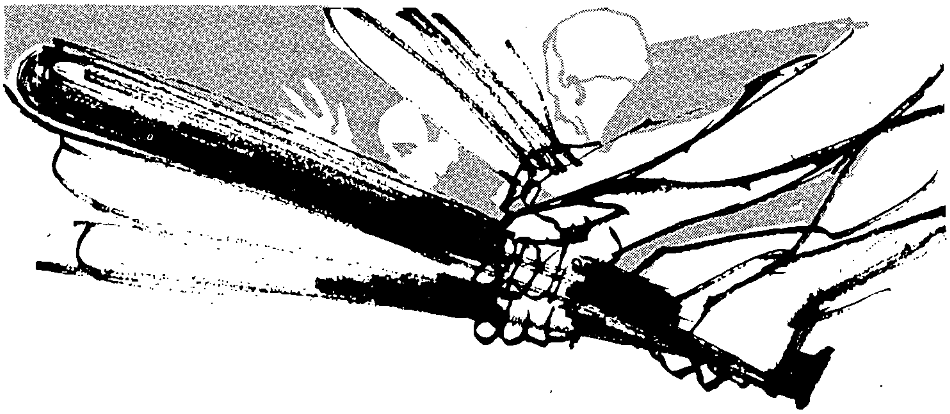

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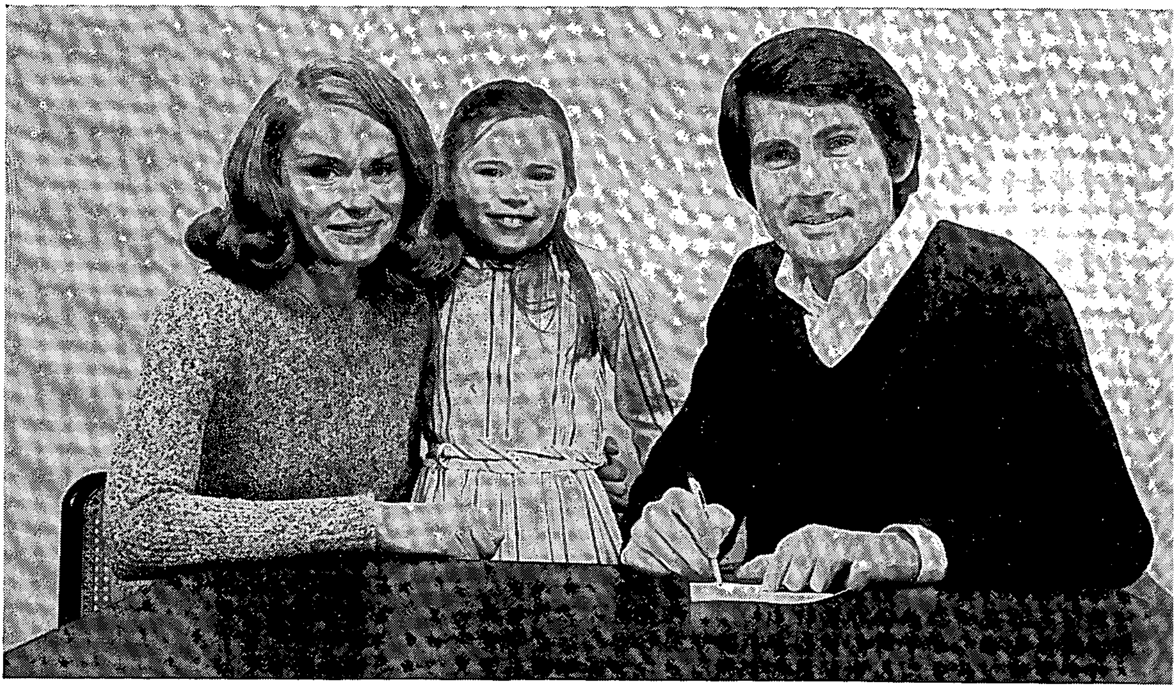

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